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ELECTRICAL MAGAZINE.

JULY 1845.

XCVII. *On the Progress, and on the Present State of Galvanography.* By FR. de KOBELL. (Translated from the *Kuntsblatt* of Oct. 22, 1844. Extracted from the report made by the Author to the Royal Academy of Munich.) Vide "*Archives de l'Electricite*, Vol. iv. p. 584, March 15, 1845.

(TRANSLATION.)

GALVANOGRAPHY has recently received some remarkable improvements; it has furnished products which have surpassed the hopes raised by the earlier experiments, and which have rendered evident the various applications to which it may be appropriated. Indeed, four years have scarcely passed away since the first galvanographic plate furnished impressions;* and we now find in the market galvanographs, some of them being of very large dimensions, relating to all subjects to which a graphic art may be employed, and presenting the most delicate execution. A circumstance very favourable to the extension of this art is, that it may be associated with all the known methods of engraving—by the cold point, or by aquafortis, with aquatint, with line engraving, &c.; and that things which cannot be executed without difficulty, in these divers arts, very often present, on the contrary, in this latter, advantages and peculiar facilities.

I purpose relating, without entering into detail, the various experiments that I have myself made for this purpose, or that I have had occasion to make by the assistance of the works of others; they will, I hope, contribute a little to extend the galvanographic art over a still wider field.

* I appended a specimen to my memoir on this subject in the 20th Vol. of *Erdmann's Journal.*

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Whilst, in the ordinary process of engraving, the figure is executed in intaglio in a plate of copper, by the well known processes, precisely the reverse takes place in galvanography, namely, the plate of copper is taken, and the image is worked upon it, after which the plate serves to give proof plates. For this purpose, the image must be drawn or painted with a single colour on a plate of plated copper. Encaustic colours—those prepared by dissolving wax in oil of turpentine, or in balsam of copal, have been found to present peculiar advantages for this purpose, because they are dead after they become dry, that is to say, they possess a certain grain, which is a fundamental condition for the fixidity of the colour, and consequently for the success of the impression, in cases when the painting is made with broad strokes of the brush, and not in lines. It commonly happens, in making these proof plates, that beginners have a fault of not allowing the image to dry sufficiently; the plate is then spoiled, because the substance that was employed to make the painting condenses, when it comes in contact with the sulphate of copper, which permits the copper to insinuate itself beneath the layer of colours; and thus a plate is produced which presents nothing more than the traces of an image, or an image which has the appearance of being half-effaced. The drying may be hastened by slightly warming it.

There is another colour which also presents great solidity; it is composed of lithographic chalk of the hardest kind, powdered and washed in distilled water. This colour, which dries very rapidly, is more easy to manage than an oil colour. Being insoluble in the solution of sulphate of copper, it does not permit the copper to extend beneath it. In commencing, we may give the plate a very thin coat of encaustic colour, and then add the lithographic colour with a fine pencil, in lines that are not too thin. The painting is very easy; and we may thus obtain portraits of very fine execution; such are those which MM. Rottmann, jun, and P. Wronski have painted and galvanographed.* After the operation, the details of the plate may be added by aqua fortis. Lithographic chalk may also be used in the form of a drawing pencil, providing that care be first taken to deaden the plate; for it is not possible to draw with a pencil of this kind upon a polished surface of metal. This deadening is obtained by melting upon the plate the substance called grain of aquatint, after which it may be drawn upon with chalk. The bright parts are easily produced by removing the grain. Drawings of

* Instead of lamp-black, we may mix with the chalk iron red, or Kassler's brown, which gives more body to the colour.

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this kind possess a great deal of softness, however the line grain, of which I shall speak hereafter, is much superior.

A similar grain may be given by making use of aquafortis, as in aquatint, instead of the resin; then, by the galvanic process, we take one impression of the plate in relief, and it is sufficient to give very slight thickness to this relief; so that for small plates we are able to obtain an impression in twenty-four hours. It is on this relief thus grained, and which is afterwards silvered that we draw or paint. In landscapes, as the air and other details can easily be produced by two tints of aquatint, there is an advantage in making them by this process, and in slightly engraving the outlines; we then finish by painting the image on the relief. It was upon this plan that M. Rottman, jun., produced four very large views of Munich, which may be found in the house of Cotta, at the expense of which they were executed.

A grain peculiarly valuable for certain objects is obtained with the fine Paris roulettes. The outline of the object is first slightly engraved; the back ground is then submitted to aquafortis, and a slight tone is given to the entire image with the roulette. In order to perform this operation in a very equal manner, the roulette is fixed in a ruler.* The shadows may also be made with stronger tones. Proofs are then taken off in order to examine the tone; the galvanic relief of the plate is taken, and the image is finished by the aid of lithographic chalk, or with the pencil; and then the plate intended for the impression is executed upon it. It is easy with a graving needle, with the point of a pencil, to remove any points of the chalk or colour that are too dark, or to make other corrections in this image. Plates of this kind furnish impressions, the effect of which presents an agreeable union of aquatint and line engraving.

When the galvanographic plate is finished, of whatever nature it may be, there is an advantage in passing the roulette over it; if this operation is carefully managed, the image does not suffer at all; on the contrary, it gains in harmony and in grain.

All the preceding details have relation to the manner of executing the drawing, the impression of which is to be taken in intaglio by the galvanic way, so that we may afterwards be able to multiply it by printing. With regard to the deposition of copper, I have not found it necessary to make any essential change in the apparatus, or in the decomposing liquid, as described in my "*Galvanography.*"† I will, however, draw

* The roulette may also be employed for the back ground of the engraving; and the tone may be given by aquafortis.

† Cotta: Munich, 1842.

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attention to a few circumstances which must be borne in mind, if we are anxious that the plates shall attain the greatest possible perfection. One thing of prime importance to this end is, that the plate obtained by the galvanic process may separate easily and without accident from the object on which the image is painted, and of which we desire to obtain the relief. This depends in a great degree on the force of the galvanic current; and I have found that a current whose force goes on insensibly increasing is that which furnishes the best results. Now, by means of the diaphragm apparatus, such as I have described it in my work, it is very easy to obtain with certainty a current of this kind; and we have nothing else to do than at the first to pour into the apparatus common pure spring water, instead of acid or dilute saline solutions. The decomposition of the sulphate of copper then begins with extreme slowness, and acquires more and more importance, in proportion as sulphate of zinc is formed in the apparatus, and as the force of the current by this means becomes more considerable. When the support on which the plate rests is itself covered with copper, which occurs at the end of twenty-four hours, the formation of the deposit of copper may be accelerated by means of the dilute sulphuric acid that had been described.

When the solution of sulphate of copper has become sufficiently saturated, the plates of copper prepared according to this method, may be detached from the support in the most complete and easy manner; this support, moreover, ought to be silvered according to the process pointed out in my writing, and the execution of which is very easy. This observation is also important to those who wish, for example, to copy by the galvanic process plates that have been engraved with the graver.

Parchment is the most convenient substance for diaphragms, and care must be taken not to plunge the apparatus into the sulphate of copper any further than is necessary for the parchment to touch the liquid. If it should happen that the zinc, placed in the apparatus, becomes dry at the end of twenty-four hours, it would indicate that the parchment is too thin, or that little holes are formed in it, through which the liquid containing the zinc has passed; these holes may be stopped with varnish, without putting the apparatus out of action. In my more recent experiments, instead of rolled zinc I have employed cast plates of the same metal; they last longer, cost less, and are more easy to clean.* A solution of sulphate of

* We have had no small experience in voltaic depositions, and have arrived at the conclusion that rolled zinc is by far the best. In London, it is not much

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copper, mixed with a third in volume of solution of Glauber's salt, is to be preferred to this solution, mixed with a solution of sulphate of zinc; for the latter sulphate in any case gradually mixes with the sulphate of copper. When, after long use, the solution no longer dissolves the crystals of sulphate of copper, and acquires a decided clear blue colour, it is a sign that it contains too large a proportion of sulphate of zinc, and that it can no longer serve to produce galvanic plates of copper.

With regard to the silvering of the plates on which we paint, it is necessary, when this operation is done, and the plates have been well cleaned with a leather and lime, that they be still kept for about twelve hours in a solution of sulphate of copper, and then dried quickly with blotting paper and linen. The yellowish tint is thus avoided, with which, in other cases, the plates are often covered.

With regard to the latest productions that galvanography has supplied to commerce, and which demonstrate the numerous modifications that the art has undergone, I will first mention, in addition to those of which I have spoken above, the productions, which are as skilfully as they are happily executed, of MM. Schœninger, and Freymann, of Munich. There are among others an *Ecce Homo*, after an original picture, forming part of the collection of the Canon Speth of Munich; the *Smoker*, after a picture by Ochterveld; a *Christ on the Cross*, after Tintoret (of very large dimensions); *St. Catherine*, after Raphael. The same artists have recently executed a *Madonna della Sedia;* and they are at the present moment occupied upon a portrait of the Princess Hildegarde.

Among the foreign galvanographs with which I am acquainted, I may mention, as being worthy of particular attention those which have come from the laboratory of MM. Theyer, and Waidell, of Vienna. They are almost all made by painting the images with a single colour, as may be recognized in the prints. This process is assuredly the most original in galvanography, and it presents peculiar advantages for landscapes, groups of trees, animals, flowers, &c. The most remarkable pieces are *The Bitch and her pups*, of A. Wengler, after the original of J. M. Ranftl; *Side door of the Church of St. Stephen, at Vienna*, by Griesser; a landscape, by Jacob Waltmann; an architectural design, by P. Lang; and *Flowers*, by an unknown artist. I may also mention a very fine sketch of a horse, which is due to the Archduke

more costly, and it lasts much longer, being far less liable to local actions.—*Ed. Elec. Mag.*

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Stephen, and which testifies honourably to the interest paid by that prince to this art.

In conclusion, I may remark that the apparatus which conform to the description that I have given in my work, and which have the necessary dimensions for small pieces, are of moderate price; and that their employment, and the processes to plate and prepare them are so easy, that none of these things can constitute difficulties capable of arresting the extension of the galvanographic art. [A]

XCVIII. *Studies of electric Photometry.* By MM. Masson. From "Les Comptes Rendus." Aug. 5, 1844. Vol. xix., p. 325.

(TRANSLATION.)

In this work, I have proposed to determine one of the constants of the formula, by which I have represented the law of the development of electric light; I have endeavoured to establish the relation existing between the quantities of light and heat, developed by the same electric current.

Finally, having measured the sensibility of the eye by a new process, I believe I have succeeded in establishing certain principles that are necessary in photometric researches.

In my first memoir,* for the mathematical expression of the development of light by electric discharge, I gave the formula.

$$I = k\,[1 + m\,(x-1]^2 \frac{s}{y^2 E}$$

I, intensity of the light; k, constant, depending on the nature of the circuit, and of the substance of the conductor; s, surface of the condenser; E, its thickness; m, a constant; y, distance of the spark from the photometer; x, distance of the explosion.

New experiments, entirely according with the former, have established that m being equal to unity, the intensity of instantaneous lights, like that of other lights, varies in inverse ratio to the square of their distance from the point illuminated; and that, in order to represent the law of the development of electric light, we must take the formula.

$$I = \frac{k\,x^2\,s}{y^2\,E}$$

According to MM. Riess and Harris, x is proportional to $\frac{q}{s}$

* Vide *Elec. Mag.* Vol. 1, p. 272.

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q representing the quantity of electricity accumulated on a condenser, whose surface is s; and as it follows from my experiments that x is also proportional to E, we shall therefore have

$$x = \frac{k\,q\,\mathrm{E}}{s};$$

and consequently

$$\mathrm{I} = \frac{k'\,q^2\,\mathrm{E}^2}{s^2} \cdot \frac{s}{\mathrm{E}}$$

This latter formula, by making E constant, represents the law discovered by M. Reiss, of the development of heat in a wire traversed by the current of a battery.

The electric tension in each point of a condenser being, according to the learned German philosopher, proportional to x^2, I think we may deduce, from his interesting researches and my own, the following consequences:—

1st. The quantities of light produced by electric discharges are to each other in the same relation as the quantities of heat developed in a wire by the current due to the discharge; one of the effects of electricity, therefore, being able to serve as a measure of the other, we can conceive the possibility of being able to reduce photometric and electric measurements to simple thermometric values.

2nd. The quantities of light and heat developed by electric discharges are proportional to the tension of the fluid on the condenser, in direct ratio to the surface of the said condenser, and are inversely proportional to its thickness.

(*Here follow experiments on the sensibility of the eye, in order to determine its value in these observations.*)—[A.]

XCIX. *On the Electric Nature of Water-spouts.* (Letter from M. Peltier.) From "Les Comptes Rendus." Dec. 2, 1844. Vol. xix., p. 1210.

(translation.)

The waterspout, that devastated the town of Cette, on the 22nd of October,* reminds us of the disasters of the one on June 18th, 1839, in the Commune of Chatenay. In both cases, the effects are completely inexplicable, if we would have recourse to whirls produced by the meeting of contrary winds. In the one locality, as well as in the other, the power, which tears up trees and transports them to a distance,

* Vide *Elec. Mag.* Vol. 1, p. 612.

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instead of beating them down; which tears off roofs, and carries their relics many hundreds of yards, sometimes even against the direction of the wind, of which I give many examples in my *Traité des Trombes;*—this power, which acts in closed apartments, which raises up their pavement or their flooring,—which pierces windows, without starring them;—this power, which is felt only along a narrow belt, beyond which a calm prevails, or beyond which scarcely a light breeze is felt;—this power, we say, cannot be the effect of violent and opposing winds, which by their enduring conflict cause the point of meeting to whirl round.

These opposed currents in the same stratum of air are physically impossible; they are superposed, but they can never encounter each other in a lasting manner; not any of the hypotheses, which are based upon the opposite conflict of winds, can stand in the face of observation; for we then take one of the effects as the cause.

At Chatenay, we saw M. Dutour on his balcony, as we have seen at Cette, M. l'Abbé Cros, on his steeple, to be present during the formation of the meteor, during its advance, and during its destructive effects in a limited zone, without danger to themselves, even up to the moment, when by its progression, it had enveloped them in its sphere of activity. We can cite a still more convincing example; it is that of the waterspout of June 19, 1794, at Northford, in Connecticut, which overturned a granary to its very foundations, in the presence of the proprietor, who was standing on the step of his door, on the opposite side of the road, and who did not feel any thing. There is nothing but electricity, and electricity of prodigious tension, which can produce effects so violent, within limits so very confined, while leaving the surrounding places calm.

We have, moreover, demonstrated in our work, by numerous citations and by direct experiments, that these violent aërial agitations within a circumscribed point are derived from actions purely electrical. Since which time we have given, in special memoirs, an explanation of the high electric tension, that a cloud may acquire, by making its interior constitution better known,—by demonstrating* the proper *individuality* that each particle of vapour preserves, in the co-operation that it lends to the formation of the first *flakes;* as well as the *individuality* of these flakes, in their agglomeration into foaming masses; and so in order to the largest *nimbus,* which possesses a special electric sphere at its circumference.

* Vide *Archives de l'Electricite,* Vol. 4, p. 173.

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It is the individual tension of each of these constituent particles that gives rise to the static tension of a cloud on neighbouring bodies, and not the single action of the general electrical sphere, which envelopes the nimbus. This latter very readily discharges on approaching terrestrial bodies; and it is this alone, by its instantaneous escape, that produces the trail of fire termed *lightning;* not any of the individual interior spheres co-operate in this discharge. The equilibrium being broken by this peripheric discharge, they produce a new sphere of electricity in the cloud, by a new interior equilibration, and thus render a second discharge possible, and afterwards a third, until finally their alternation is no longer able to give a sufficient charge to the circumference.

In his luminous analysis, on Monday last, M. Arago made it perfectly evident that the authenticated facts of the waterspout at Cette could not be understood without the aid of electricity; such an opinion is of high value, and we hasten to record it.

Before terminating, I must revert to a fact of great importance in this question,—a fact, from which I was not able, in 1839, to deduce all the instruction it contains; it is that of the almost complete desiccation of 850 trees, which were rent into shreds at Chatenay. I deduced from the said fact, that this longitudinal cleavage could only arise from the instantaneous evaporation of the sap by a powerful electric current; and that these trunks had yielded to the elastic force, in the direction of their least resistance, namely, in the direction of their length. Not having been called to the place until a month after the event, this desiccation might be attributed, at least in part, to the high temperature that had prevailed during this month; although this high temperature left the cleavage unexplained. But the analysis, that I had not been able to make in an opportune season, was made by M. d'Arcet, two or three days after this disaster, which I did not learn until after the publication of my treatise. This learned academician communicated to me the result of his experiment, in the presence of M. Gay Lussac. "The trees that were standing," said he to us, "possess from 36 to 44 per cent. of water; those, which have been down for four or five years still possess 24 or 25 per cent.; whilst the trunks, cleaved at Chatenay, do not contain more than 7." This result removed all doubts; these trees had had the greater part of their sap reduced into electric vapour; and this instantaneous vaporization could only arise from a powerful electric current. No second explanation is possible.

I have thought that these details would not be devoid of

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interest at the present moment; and that it would be useful to recall to mind that we cannot judge of such meteors, except by an attentive comparison of the various effects which they present, according to the seasons, and the localities; and that we must also keep in view the similar effects that arise from purely storm clouds, and those which arise from experiments.—[A.]

Note.—We are not in possession of M. Arago's observations, alluded to by M. Peltier; but we perceive that, at the meeting of the Academy, on Monday, Nov. 25, it was resolved that an inquiry should be made into the characteristic features of the waterspout at Cette, under the directions of the Physical section, with the assistance of M. Arago. We have not yet met with the results of this inquiry.

On Monday, Dec. 16, a note was read on this subject from M. Clerget, in which he states that, though some of the fragments at Chatenay, as those found by M. d'Arcet, had lost much moisture, others found by himself possessed as much sap as did pieces cut from a standing tree.

M. Arthur wrote, proposing to explain the phenomena, without the intervention of electricity; and in a letter read at the following meeting (Dec. 23), he attributed all the phenomena to differences of atmospheric pressure.—*Ed. Elec. Mag.*

C. *Table of Corrections for the Quadrant Electrometer.* By Signor Belli. From Zantedeschi's Treatise on Magnetism and Electricity, Vol. i., p. 78.

Degrees indicated by the Electrometer.	Correct degrees.	Degrees indicated by the Electrometer.	Correct degrees.
1°	3	16°	16·1
2	4	17	17·0
3	4·9	18	18·0
4	5·8	19	19·0
5	6·7	20	20·0
6	7·6	21	21·0
7	8·4	22	22·0
8	9·3	23	23·1
9	10·1	24	24·2
10	11·0	25	25·4
11	11·8	26	26·6
12	12·6	27	27·9
13	13·4	28	29·2
14	14·3	29	30·5
15	15·2	30	31·8

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Degrees indicated by the Electrometer.	Correct degrees.	Degrees indicated by the Electrometer.	Correct degrees.
31°	33·1	46°	57·9
32	34·4	47	60·4
33	35·7	48	63·0
34	37·0	49	65·8
35	38·4	50	68·9
36	39·8	51	72·2
37	41·2	52	75·8
38	42·7	53	79·7
39	44·3	54	84·0
40	46·0	55	88·7
41	47·7	56	93·8
42	49·5	57	99·4
43	51·4	58	105·6
44	53·4	59	112·5
45	55·6	60	120·0

CI. *On the Re-Electrometer as a Means of Discovering the Direction of Lightning.* By M. MARIANINI. From a Letter to Dr. Boncompagni, in the "*Annales des Sciences de Bologna*, April and May, 1843. Translated into "*Les Annales de Chimie et de Physique*, Feb. 1845. Third Series. Vol. xiii., p. 245.

(TRANSLATION.)

I HAVE demonstrated that by means of the re-electrometer,* we are able to turn aside from an excellent metallic conductor, a part of the ordinary electrical current that traverses it. I believe that we might obtain the same results by operating upon the atmospheric electricity of lightning. For this purpose, I propose, by means of two long wires, to place the two extremities of the helix of the re-electrometer in communication with two points of an ordinary lightning conductor, at no great distance apart. I would place in the glass tube of the re-electrometer a bundle of iron wires, and beneath this a compass needle, so arranged that the axis of the tube makes a right angle with the magnetized needle, and that the middle of this axis, and the centre of the needle are in the same vertical. Thus every time that lightning passes down the conductor, a small portion of the electricity will circulate round the iron wires, and will magnetize them; and the deviation of the needle that is placed beneath, will indicate the direction of the magnetization,—that is to say, will make

* Ann. de Chim. et de Phys, third series, vol. x., p. 497.

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known whether the lightning was ascending or descending in the lightning conductor.

This indication will be more certain than that deduced from the electric state of the atmosphere before the flash of lightning; for it frequently happens in storms, that the electric state of the atmosphere rapidly passes from positive to negative, and *vice versâ*. Moreover, it is not proved that, from this electrical state of the atmosphere such as we have been able to establish it, we may be able to deduce with certainty that of the storm-cloud. Besides, the indication given by my instrument will have the advantage of being capable of observation without hurry; for the magnetization produced by instantaneous currents does not disappear, at least in great part, with the same rapidity as that arising from voltaic currents.

Since we can render the fraction of electricity that is turned aside from the conductor as small as we please, and this by taking the two points of departure of the wires communicating with the helix very near to each other on the lightning conductor, it might still, however, be feared that this small fraction would be able to melt the wire which it traverses. To avoid this inconvenience, the instrument may be used in another manner, namely—the two extremities of the helix of the re-electrometer might be made to communicate together by means of a wire, thus constituting a closed circuit; about one metre of this wire might be in a direction parallel to the lightning conductor, and at a distance of several metres. I am convinced, by my own experiments on the spark of the electrical machine, that the re-electrometer would indicate the passage of lightning in the lightning conductor; and in this case, the re-electrometer being electrized by induction, its indications would evidently be in a direction contrary to those which the instrument would have given, had the wire been traversed by a direct portion of electricity. It is very true, as we have seen, that when the instantaneous currents act by induction, they produce magnetization, sometimes in one direction, at other times in the opposite; but I have already observed in the preceding memoirs, that when the tension is great, the induced current always has the same direction; and that if the iron is altogether devoid of magnetism, it will be always magnetized in the same direction.

It is possible that by this method, and merely by augmenting the length of the part of the wire, that is parallel to the conductor, the re-electrometer may indicate a lightning flash at notable distances; for if a small Leyden jar has given us inductions at the distance of 80 cent., may we not

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conclude that lightning will give them to us at the distance of several metres?

I am even bold enough to hope that we might obtain a magnetization by induction, when lightning bursts merely between clouds; for Leyd-electric inductions take place even when the inducing conductor is nothing more than a stratum of air.—[A]

CII. *Resumé of certain Researches concerning the Action of the Cyanurets of Ammonium, Calcium, Magnesium, and Barium on the Ammoniuret and the Chlor-ammoniuret of Gold, and on the Auric Hydrate.* Extract from a letter addressed by M. Selmi to the Editor of "L'Institut." No. 578, Jan. 21, 1845. Vol. xiii., No. 4.

(TRANSLATION.)

While engaged in galvanic gilding, I had occasion to study the action of certain simple cyanurets upon the ammoniuret and chlor-ammoniuret of gold, and upon the auric hydrate. The following is the summary of the principal propositions which are deduced from the entire work:—

1st.—The cyanurets of ammonium, calcium, barium, and magnesium, possess the property of dissolving with rapidity, both by heat and cold, the moist and recently prepared ammoniuret and chlor-ammoniuret of gold, of decomposing them and giving off ammonia.

2nd.—The same cyanurets are in part decomposed during the reaction; for the auric acid of the ammoniurets is transformed into cyanuret, giving up its oxygen to the alkaline or feroso alkaline radicle, which remains in a caustic state in solution in the liquid, providing it be soluble; or is else precipitated, if it cannot be dissolved in the vehicle; in this respect, the ammonia that is separated from the ammoniacal cyanide is at first dissolved in the liquid, and is then gasified, whilst the lime, magnesia, and baryta of the cyanurets, corresponding to their metallic radicles, are deposited at the bottom of the vessel in which the operation has been conducted.

3rd.—By means of the reactions pointed out above, are formed double cyanurets, aurico-ammoniacal, aurico-calcic, aurico-barytic, and aurico-magnesic, which are very soluble in water, soluble in alcohol, crystallizable, colourless, undecomposable by carbonic acid; they possess the property of gilding

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magnificently metallic objects with M. de la Rive's diaphragm apparatus.

4th.—The aurico-calcic, aurico-barytic, and aurico-magnesic double cyanurets are partly decomposable by ammonia; the two former more evidently, if it be added to alcoholic solutions; the third very distinctly without the addition of this liquid; moreover, the latter, when exposed to the air until the total escape of the volatile alkali is re-composed, and the magnesia is gradually re-dissolved by again substituting itself for the ammonia.

5th.—The four simple cyanurets above mentioned, dissolve metallic gold in a state of division, when it is digested with their solutions.

6th.—The auric hydrate, when moist and recently prepared, dissolves in the four simple cyanurets, producing corresponding double cyanurets; but the solution of the same is brought about with much greater difficulty and slowness than for the ammoniurets of gold. This resistance to solubility is more marked with the ferroso-alkaline cyanurets than with the ammoniacal cyanurets.

The phenomena that have just been related, seem worthy of the attention of chemists, for they present us with rare examples of reactions, in which an insoluble saline substance (the ammoniacal aurates, or ammoniuret of gold), when in contact with a binary in solution, is decomposed into acid and base, the latter remaining at liberty, the former reacting successively on a portion of the binary with a double decomposition, and a mutual exchange of radicles. The new combination, of which the radicle of the acid of the saline body forms a part, is of an acid nature, and is associated with the undecomposed part of the binary, whilst the other combination, in which is found the metallic radicle of the binary, remains at liberty.

Finally, the feeble resistance which the ammoniacal aurate opposes to the decomposing action of the alkaline cyanurets, as compared with the auric hydrate, and in contradiction to what one would have presumed, is a fact which merits some attention. This anomaly seems related to the known quality of the same ammoniacal aurate of being resolved into its components, both by percussion and heat, much more rapidly than the auric acid.

Fr. Selmi.

Reggio, Nov. 1844.

[A]

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CIII. *Note on a Particular Light, which is frequently manifested by Night in the Northern part of the Sky.* By M. Colla, Director of the Observatory at Parma. From "Les Comptes Rendus," Feb. 3, 1845. Vol. xx., p. 323.

(TRANSLATION.)

In 1829, an anonymous observer wrote to the Baron of Zach, (*Biblioth. Univ. de Genève*, t. xlii, p. 271) that, while occupied day and night for six years in observations on the transparency of the atmosphere, he had proved the existence of a singular light, that was manifested by night towards the north-west and toward the south-east. This light, which had, he said, some resemblance to the zodiacal light, differed from it in that it was manifested in the direction of the magnetic meridian. Always more apparent on the side of the north-west than toward the opposite point, it was even sometimes manifested with a sky entirely clouded; and in this case one might have been able to take it for the reflection of a distant fire. The author of the letter remarked that, in order to recognise the reality of the phenomenon, it was necessary that the observer should be placed so as to have his horizon perfectly open; and, moreover, that he should be accustomed to recognize feeble variations in the brightness of the sky.

When I read this letter, I was the more struck with it, inasmuch as, on my part, from the year 1825, I had several times observed this phenomenon, and had frequently made mention of it in my registers of observations; I had even several times announced it in journals and in my *Annuaires* under the denomination of *Impressione luminosa verso penente estivo.* My observations differed, on the other hand, from those of the anonymous writer, in that I had never seen the light toward the south-east; and that it very rarely appeared to me in a cloudy sky.

Observations made in June, and in a part of the months of May and July, have proved to me that it is sometimes a very difficult matter to determine the existence of the phenomenon, because at this period the light in question is confounded with the crepuscular light which remains almost the whole of the night, and with the zodiacal light, which at that time is manifested under the form of a zone in the northern horizon, between the east-north-east and the west-north-west. Another circumstance, equally unfavourable to the observation of the phenomenon, is the clear brilliancy of the moon.

That this phenomenon arises from a cause quite different from the northern light is incontestibly proved by many cir-

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cumstances: 1st, because it is sometimes visible through the whole length of the winter nights; 2nd, because it is constantly manifested toward the north; 3rd, because it presents at all periods of the year its greatest luminous intensity, almost in the direction of the magnetic meridian.* This light, moreover, must not be confounded with that white space of an arched form, which marks the limit between the morning and evening twilight; because the latter, even under the most favourable circumstances, is never visible throughout the night, whilst the luminous impression, of which I speak, is permanent, at times, throughout the entire night.

Under certain atmospheric conditions, this light, which may be designated the *magnetic light*, appears more extended. From white, which is its usual character, it takes a slightly yellowish tint; and in this case it commonly acquires superior brilliancy; it was when it presented itself to me under this form that I noted it in my registers, and announced it by the title that I have just mentioned. The form, which it generally affects is that of a zone, parallel to the horizon, and 10° or 12° wide. Since I have had at my disposal a very sensible dipping needle, which enables me to estimate a second (*un minuto secondo*) of variation in an arc,† I have been able to prove that the augmentations in the intensity of this light correspond with the days, when the disturbance in the march of the needle are more or less sensible. It would therefore appear that this phenomenon, like the aurora borealis, is connected with the phenomena of terrestrial magnetism.

I have said that, in a few rare cases, the phenomenon is even observed in cloudy nights; but for this it is necessary that the moon be not visible, and that the sky be not a stormy sky. Moreover, the zodiacal light, which is also very feeble, and which, in relation to its luminous intensity, very much approaches the light of which I am speaking, sometimes also appears through the clouds, when they are not too thick,‡ which was proved, for example, in divers places in Europe, in March, 1843.

The singular phenomenon of the boreal light, on which we

* The magnetic meridian at Parma is about 17° 15′ west of the astronomical meridian.

† This compass, which I had at my disposal in November, 1843, was constructed at Parma by M. Grinelli.

‡ Mr. Wartmann, in a letter of March 31st, announced this observation to me in the following terms:—"Since the commencement of this month, an aurora borealis has been manifest of an evening from the west horizon to the hyades, every time that the sky was clear, and sometimes even through clouds, when they were not too thick."

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are engaged, several times presented itself to the two talented observers MM. Quetelet and Wartmann, who have mentioned it under the name of *feeble aurora borealis*, or *doubtful aurora borealis*. Without doubt, to this phenomenon must we also trace the indication given in Garnier's *Treatise on Meteorology*, chap. xvi., p. 430-1, of an observation made in the United States, on Nov. 17, 1835.

We would further remark, that, if this light has hitherto passed unperceived, there is no cause to be greatly astonished; since, on the one hand, it is always very feeble; it is scarcely visible to those who are not accustomed to seek for it, except under particular atmospheric circumstances; even when the sky is perfectly open, and the moon does not shine, it has sometimes very little intensity; and finally, the observer must be so placed as not to have his horizon obstructed on the north-west side.* Finally, I would add that what happens in this phenomenon is nothing more than the actual repetition of what took place in respect to the zodiacal light, the observations of which, as it appears to me, must be traced back to a very ancient period, although we find none anterior to those made by Cassini, in 1683; and that even, since 1694, no one paid further attention to it until the period when Mairan, about the 18th century, devoted himself to researches on the cause of auroræ boreales.

In terminating this note, I ought to say, that I have already endeavoured to call the attention of philosophers to the phenomenon in question. M. Bianchi, an astronomer at Modena, was good enough, at my solicitation, to make it the object of communication to the Scientific Congress, which was held at Milan last autumn. He invited the astronomers and philosophers who were present at that assembly to repeat my observations. I now address the same demand to the French philosophers. Should they establish, as I believe they will, the reality of the phenomenon, I hope they will make good use of the means of publicity, which are at their disposal, so as to make known the results of their researches, both on the fact in itself, as well as on the causes to which they may trace it.—[A.]

* I have found that the best means of determining the existence of this light, in cases when it is very feeble, is to turn toward the opposite part of the heavens, and to bend backward until we are able to see, with the head inverted, the portion of the horizon, where the phenomenon is accustomed to be manifested.

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CIV. *Electrolysis applied to Toxicology.* By MM. DANGER and FLANDIN. From "Les Comptes Rendus," March 31, 1845. Vol. xx. No. 13, p. 951.

(EXTRACTS.)

IN the toxicologic study of mercury, the only progress due to our epoch is Smithson's discovery of the employment of the pile. The elements of this little apparatus are well known, being a plate of tin, surrounded by a plate of gold formed into a spiral. The tin forms the electro-negative (pos. Qy?), and the gold the electro-positive (neg. Qy?), element. When plunged into a solution containing mercury, this pile separates from it the metallic element, which is reduced on the gold and whitens it; all that is then necessary is to volatilize the metal in a small tube, so as to obtain it in the very characteristic form of a liquid globule.

By a comparative examination of the reactions proper for detecting mercury in its solutions, we have satisfied ourselves that galvanic, or galvanoplastic action is the most sensible. We have been enabled, by means of this reaction, to establish the existence of mercury in a solution diluted to a hundred thousandth. This is the limit upon which we have fixed in our preceding researches. We are fortunate in not having any exception to make in respect to a metal which is not only a formidable poison, but which, under divers forms, is a valuable medicine, and one frequently employed.

In our experiments, we did not make use of the galvanic apparatus, such as it has been described by Smithson; we merely retained the principle. For toxicologic researches, this ingenious instrument would have presented inconveniences that we were desirous of avoiding. We propose substituting the following apparatus for that of the ingenious English chemist.

A vessel, V, serves as a recipient for the liquid under examination; upon a support, S, of particular mechanism, is adapted a kind of funnel, E, terminated by a drawn-out tube, F, the opening of which is almost capillary. The escape tube forms with the body of the funnel an angle of 90°. The vessel, V, being filled with the suspected liquid, is inverted in the little funnel. By means of an articulation in the support, we may give the apparatus, as it stands, such an inclination as we deem suitable for the escape of the liquid. In the wide part of the funnel, E, is placed the electro-negative conductor of a pile of one pair of Bunsen's, and in the opening of the capillary tube, F, is introduced the electro-positive conductor.

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Both wires, in the part at least which touches the liquid, ought to be of pure gold. The two poles must be brought so near together as almost to be in contact. In consequence of the excess of pressure on the capillary opening of the tube, F, the liquid makes its escape drop by drop, and is received in another vessel. Since a vessel, V, performs the functions of Marriotte's vase, or of an intermittent fountain, the pressure remains constant on the liquid, and the escape is regular to the point, G. The escape may be accelerated or retarded, at the pleasure of the operator, by the degree of inclination given to the apparatus; it has appeared to us that, in ordinary cases, it ought to be so regulated that each drop of liquid in falling should mark an interval of five seconds. The pile being placed in action, a more or less abundant escape of gas occurs at the poles, which is an indication of the intensity of the current, and the mercury of the solution is deposited upon the electro-positive (?) gold wire, and whitens it. To be satisfied that this coloration is due to the mercury, it only remains to volatilize the metal in a small reducing tube by means of a small spirit lamp.

Being assured that with this apparatus we could discover the most feeble traces of mercury that chemistry could recognise, it remained for us, in order to attain the object we had in view, to find a fit process for separating mercury from organic matter.

(*As this is purely a chemical process, we must pass it over, referring such as are interested in it to the original memoir.*— Ed. Elec. Mag.)

Experiment has demonstrated to us that the voltaic current favoured the precipitation of mercury on the gold wire; and that, in all cases, it at least had the advantage of accelerating an operation which, without the assistance of this action, would perhaps have required a considerable time for its accomplishment.

The metal being obtained on the electric-positive (?) conductor of the pile, it is necessary, in order to remove all fatty matter, to wash the gold wire in boiling ether or alcohol, and to dry it before introducing it into the reducing tube. The latter ought to be prepared and blown with the requisite precautions for avoiding the moisture that would sully the globule of mercury, which is sometimes extremely small, and which we desire to make visible to the eye.

This process has already been subjected to proofs, suited to enable us to form a judgment of its value in cases of poisoning. We have not required more than 100 grammes (1543 grs. troy) of the liver of an animal poisoned by sublimate, in order

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to be able to extract the mercury in an appreciable quantity. Henceforth there will not be more difficulty in following the traces of crime committed with powder of succession, or corrosive-sublimate, than those of poisoning by arsenious acid, or any other metallic compound.—[A]

In consequence of the above communication, one of the journals of the day had stated that a certain rob ought to be condemned by the civil authorities, because Smithson's pile had detected mercury in it. M. Orfila, at the meeting of the Academy, on April 7th, reminded the members that Smithson's pile when used, as in the above case, according to the inventor's directions was a faithless apparatus; and alludes to a publication of his own, in 1829, in which he had pointed out the various errors to which this arrangement was liable,* as well as the precautions to be taken to avoid them, and which were exactly the same as those just pointed out by MM. Danger and Flandin. He further added that these gentlemen had committed some grave errors; and that juries and magistrates were quite in a dilemma as to what should be accepted as true, and what not; and he entreated the Academy to hasten the presentation of the Report of the Committee upon the labours of MM. Danger and Flandin.

At the following meeting, April 14th, MM. Danger and Flandin explained that M. Orfila's misconception of their statements arose from his not having seen the words of their memoir itself, in which there is no indication of their having appropriated his discoveries to themselves. As he had not explained what these "grave errors" were, it only remained for them to join him in his wish that the report of the Committee might be handed in as soon as possible.

CV. *New Researches on Animal Electricity: on the Muscular Current, and on the Proper Current.* (Extract of a letter from M. MATTEUCCI to M. de Humboldt.) "Comptes Rendus," April 14, 1845. Vol. xx. No. 15, p. 1096.

(TRANSLATION.)†

IN order to complete all that relates to the muscular current, I must first mention that I have very distinctly obtained signs of tension with the condenser at the two extremities of my muscular piles. I have also obtained signs of *electro-chemical decomposition* by the muscular current. That which particu-

* Vide *Annales de Chimie*, vol. xli. p. 92, 1829.

† This paper has also been translated into the *Philosophical Mag.* for June.

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larly interested me in these new researches was to study, in a much more complete manner than I had hitherto done in my preceding labours, on the one hand, the relation between the intensity and the duration after death of the muscular current; and on the other, the activity of respiration and the circulation of the blood, the temperature of the medium in which the animal lives, and its rank in the animal scale. I have laboured at this for five months, every day submitting to experiment a certain number of frogs, that had been taken from the same pond. Of these frogs, some were immediately killed, in order to obtain a measure of the muscular current; others were placed at the temperature of the external atmosphere, in an apparatus by means of which I could know the quantity of carbonic acid gas given out by a frog in a determinate time; others, finally, were placed in an ambient medium, the temperature of which was constantly 16° (61° Fahr.). I thus operated on frogs that had lived from—4° (25° Fahr.) to 16°. The result of so great a number of experiments left me not the smallest doubt as to this conclusion,—the intensity of the muscular current is in proportion to the activity of respiration. I operated in like manner upon frogs which had been preserved for a greater or less length of time in water *deprived of air*, and which, therefore, were in a more or less decided state of asphyxia. I always arrived at the same result.

By operating upon several warm-blooded animals, I verified in a more complete manner the result to which I had already arrived, namely, that the intensity of the muscular current is proportionate to the rank of the animal in the scale of beings; whilst the duration of this current after death varies in an opposite ratio. I wished to study the influence of different gases upon the intensity and duration of the muscular current. For this purpose, I arranged an apparatus that permitted of my having a muscular pile in a certain gaseous medium, with power to open and close at pleasure the circuit of this pile with the galvanometer. I operated thus in atmospheric air, in oxygen, in very rarefied air, in carbonic acid, and in hydrogen. In these different media the muscular pile acted equally, both as regards intensity and duration. Hydrogen gas alone presented a singularity which could not have been anticipated before the experiment. This singularity is not referrable to an action of the gas on the muscles, but rather to a phenomenon of secondary polarity, which is verified, whatever be the source of the current. The fact is, that on operating in this gas with a muscular pile, the deviation remains constant for several hours. This nullity of action of the different gases named,

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upon the intensity and duration of the muscular current, plainly proves that the origin of this current is in the muscle itself, whether living or taken from the animal a short time after death. This same conclusion is rendered evident by another experiment. I prepared, with very fine intestinal membrane, a great number of small conical cavities; these cavities I filled with fibrine separated from the blood of a recently killed ox; I rapidly prepared with these elements a pile that was in appearance similar to my piles of half thighs. I obtained no sign of a current from this pile. This pile acted with the same result both in oxygen and hydrogen. It is, therefore, in the muscle, and consequently in its organization, and in the chemical actions which are going on within its very structure, when it belongs to a living or to a recently killed animal, that the cause of the current exists.

The most curious results to which I have arrived in these latter labours, are in relation to the proper current of the frog. I am now able to affirm *that this current does not belong exclusively to the frog*, but that it is manifested in all the muscles of all animals, provided that these muscles present at their extremities an unequal tendinous termination. All the muscles that have on one side the tendinous extremity more compacted, more condensed than on the other, give the current directed in the muscle from the tendinous extremity to the surface of the muscle. I have verified this result on all the muscles of the frog, on the muscles of the superior as well as those of the inferior limbs; on the muscular masses of the pigeon, the rabbit, and the dog. If I have rightly understood the latest anatomical labours made upon the structure of muscles, or their relations with the tendons and the sarcolema, I cannot hesitate in regarding the proper current, or that from the tendon to the surface of the muscle, as the most simple case of the muscular current. The tendinous fibres are continued among the muscular fibres, whilst the sarcolema merely envelopes the said muscular fibres. This result is rendered still more probable when we call to mind that the same laws preside over the proper current and the muscular current.—[A.]

CVI. *Experiments upon the Electric Discharge of the Jar.* (Extract of a letter from M. MATTEUCCI to M. Arago.) "Comptes Rendus," April 14, 1845. Vol. xx. No. 15, p. 1098.

(TRANSLATION.)

HAVING lately had occasion, in one of my lectures on elec-

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tricity, to put in action Armstrong's new electrical machine, I believe that it would be somewhat interesting to repeat Colladon's experiment with this machine. The great quantity of electricity developed by this machine made me hope that, even with an ordinary galvanometer, I might obtain signs of the current by connecting together *the insulated boiler*, and the pointed conductor, *against which* the jet of vapour is thrown out, and which is *also insulated*. And indeed with an ordinary galvanometer, the wire of which made 200 turns, and which was furnished with a very imperfect static system, I obtained, by operating in the manner above mentioned, a fixed deviation in the direction given by the conditions of the experiment. This direction occurred in the opposite way, on inverting the position of the extremities of the galvanometer, being always directed from the extremity of the wire turned toward the vapour to that of the boiler. I attempted some experiments, in order to compare the intensity of the current with the tension of the steam in the boiler: and I operated successively from two to five atmospheres. The fixed deviation increased from 3° to 10° and 27°, in going from the pressure of two to four atmospheres. On holding one of the extremities of the wire with an insulating handle at a certain distance from the conductor or from the boiler, whilst the other extremity of the wire was in contact with the boiler or the conductor, there was still a constant deviation in the needle, but it was always less than that obtained when there was no interruption of the circuit. In this case, even at a distance of several centimetres, there was a series of sparks that appeared without any interruption. I wished to convince myself of this by one of the very ingenious means discovered by Mr. Wheatstone. It was a revolving disc, on which I had traced black radii. The space of time that was occupied in passing from one radius to another, was 0·00009 of a second. The disc was seen to be immovable. It is therefore plainly proved that the light was not continuous, and that it was a series of sparks or successive discharges which traversed the circuit, giving to the said circuit the properties of a conductor, traversed by a voltaic current. The enormous quantity of electricity produced by this machine under all atmospheric circumstances, enabled me to make a series of experiments which complete the identity of the electric current, properly so called, with the discharge of the jar. I prepared a demi-rectangle of copper wire, altogether similar to Ampère's mobile conductor. The two small cups, filled with mercury, into which the points of the mobile conductor plunge, are fixed on a stem of resin. The mobile conductor is supported

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by a silk thread without tension. I fixed on a resin stand a copper wire, which was consequently parallel with the longest side of the rectangle, to which it might be approximated, or the reverse, at pleasure. The whole apparatus was covered with a bell glass to prevent the effect of the agitation of the air. It is not difficult to conceive the arrangement of the experiment, so as to succeed in passing the discharge of a battery in the two conductors, sometimes in the same direction, at other times in the opposite; I shall therefore not stop to describe them. I commenced by passing the discharge in one of the conductors alone, holding the other either insulated or else in communication with the ground; sometimes the discharge was passed along the mobile conductor, at other times along the fixed. When the two conductors are at the distance of 15 or 20 mm. ($\frac{3}{4}$ in.) on employing the discharge of a battery of nine jars, each of which presented 0·12 sq. metres (14 sq. yards) of surface, no movement was observed in the mobile conductor, even when viewed with the eye-piece of the cathetometer. At a less distance than 15 mm. between the conductors, we always saw, under every circumstance, the mobile wire slightly attracted by the fixed conductor. This attraction is manifested in a much more evident manner when either the fixed or the mobile conductor is in communication with the conductor of the electrical machine. Finally, if the fixed conductor is electrized with sparks, the mobile conductor is successively attracted and repelled. These phenomena are very easily explained by the ordinary attractions and repulsions of electrized bodies in presence of bodies in the natural state. I wished to try these experiments with my apparatus, merely to see what position it might occupy in the phenomena that I am about to describe.

The two conductors were so arranged that the spark would pass along them in opposite directions. The distance between them was from 10 to 15 to as much as 30 mm. I began by observing the mobile conductor with the eye-piece; but there was no necessity for this, for the repulsive motions that occurred in the mobile conductor at the moment of discharge are so great that they may be observed at once. On making the current pass in the two conductors in the same direction, we also saw, and that in a very distinct manner, the mobile conductor rush, at the moment of the discharge, upon the fixed conductor. So that, without any kind of doubt, we may admit that Ampère's fundamental law of the attraction of currents in the same direction and of the repulsion of currents passing in opposite directions, is verified for the discharge of the jar under similar circumstances.

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I may finally add that I again repeated my experiments on the induction of the charge of a jar,* by making the discharge of a battery pass through a flat spiral opposite to a similar spiral, the two extremities of which were united with the ends of a galvanometer wire. At whatever distance the two spirals are placed, the induced current, which always diminishes in proportion as the distance increases, is always determined in the same direction as the current of the jar. I insist on this result, because by employing the processes of magnetization in order to judge of the direction of the induced current we sometimes find this result, and at other times the contrary, according to the distance of the two spirals, and the tension of the discharge. When the induced current is made to act upon another spiral whose extremities are in communication with the galvanometer, the induced current of the second order that is obtained is determined in a direction contrary to the inducing current. These results are described in my memoir alluded to,—results that I have very recently had occasion to verify.—[A.]

CVII. *On the Sound given by a Bar of Iron, surrounded by a Spiral, at the Moment when the Circuit is closed or opened.* By Prof. C. MATTEUCCI. Vide *Il Cimento*, Nov. and Dec. 1844, p. 495.

(TRANSLATION.)

FREQUENT mention has been made of a sound given by a metal wire when traversed by an electric current. The celebrated Fabroni relates having obtained a distinct sound by making the current pass through a conducting wire, and that an iron wire is better suited than any other to the success of this experiment.

The recent experiments of Wertheim,† described in his excellent work on elasticity, have placed beyond doubt that the passage of a current in a metal wire diminishes its co-efficient of elasticity, independently of the effect of heat produced by the said current. It is easy to succeed in this experiment, it merely requires that a current be passed through a metal string stretched on a sounding-board, at the moment when it is producing a sound by longitudinal vibration; the sound is instantly perceived to fall and rise again with difficulty, when

* *Ann. de Chim. et de Phys.*, 3d Series, vol. iv. Feb. 1842.
† Vide *Electrical Mag.* vol. i. p. 605.

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the current ceases. Wertheim has also obtained a slight diminution in the co-efficient of elasticity of soft iron and of steel, when magnetized by a spiral traversed by an electric current.

In the *American Journal* of July, 1837, Dr. Page describes an experiment in which he obtained a sound from a magnet, one pole of which was near to a flat spiral at the moment when the current was made to pass or was cut off from this spiral.

(*M. Matteucci here describes Mr. Marrian's experiments, which have elsewhere* been presented to our readers, so that we may pass on.*)

I must first of all mention that, in order to obtain this sound in a sufficiently distinct manner, and without its being necessary to apply the ear to the bar, it is necessary to prepare upon a glass tube of the diameter of about one-third of an inch, a spiral of copper wire $\frac{1}{25}$ inch in diameter, covered with silk. A pianoforte string, No. 8, is stretched on the sounding-board, after having been passed through the glass tube above mentioned. The tube is so supported with wooden props that the iron wire does not touch its sides. The wire is kept in a state of tension by a weight of 9 lbs. If we pass a current from a pile of 10 Bunsen's elements, interrupted at very close intervals of time, a distinct sound will be heard at the distance of 6 or 8 yards every time the circuit is closed or opened. The value of the sound thus obtained is not so easily estimated; and even with the assistance of a person with a very delicate ear, we did not succeed in removing all doubt as to its value. We did not perceive any difference between the sound occurring at the opening and that obtained at the closing of the circuit. On varying the intensity of the current from 4 to 20 of Bunsen's pairs, the sound always retained the same value; its intensity alone varied. This undoubtedly underwent an increase from 4 to 10 pairs; beyond this number, it was found gradually to diminish, so that the current of 20 elements gave a less intense sound than that produced by 10 elements. If the weight by which the wire was kept in tension, was varied from 9 to 36 lbs., the sound still remained the same; and its low intensity rose with the increase of the weights which held the wire in tension. This experiment is sufficient to prove that the vibrations to which the sound is due are made in a longitudinal direction. Having obtained, by means of Masson's wheel, extremely short intervals of time between the closing and the opening of the circuit, the sound given by the string became more distant up to a certain limit, having passed which, the sound no longer became more

* Vide *Electrical Magazine*, vol. i. p. 527.

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distinct. It seemed to me that when 40 magnetizations, and 40 demagnetizations were brought about in a second and a half of time, the sound was as distinct as possible. The intensity of the sound also increases by introducing a second spiral with the first, and arranging this also in the given direction of the current; the variation, however, only consists in the intensity. By keeping the circuit in one spiral always closed, and then operating with the other in the usual manner, the sound is equally produced. I repeated these experiments on a large bar of iron with the same results. By making the current pass in the two spirals in an opposite direction, the sound was no longer to be distinguished. Having prepared a rod of soft iron, 10 feet in length, and $\frac{2}{3}$ inch in diameter, bent into a horse-shoe form, I held the two faces of its extremities opposite to the extremities of a very strong temporary magnet, but did not succeed in distinguishing any sound from the first rod, on magnetizing and demagnetizing it with the temporary magnet. On the other hand, on surrounding the extremities of the said rod with a spiral, the sound is distinctly obtained with the accustomed circumstances. Perhaps this effect is due to our never obtaining with the temporary magnet so strong a degree of magnetism in the rod of soft iron as when this rod is magnetized by the passage of the current. Finally, I filled the glass tube, around which the two spirals were placed, with iron filings; and with this arrangement, on applying the ear to one extremity of the tube, a distinct sound is obtained on closing and on interrupting the circuit.

I will terminate by saying that I in vain endeavoured to obtain signs of the current in the spiral, by making the wire which passes through its centre vibrate in any manner. I noticed, however, that on operating on a larger bar, I then obtained distinct signs of a current at every blow or vibratory motion that was communicated to the bar. In the production of this effect, it is necessary that the bar be resting from recent magnetization; when, at every blow given to the bar, there are signs of current in the spiral, such as would be obtained by communicating to the bar a magnetism contrary to that which it had. This experiment is easily made by wrapping two spirals on the same bar, the conductors from one of which terminate at the extremities of a sensible galvanometer, whilst the other spiral serves for the passage of a current from a pile. If, whilst the bar is in a state of magnetization, by keeping closed the circuit of the spiral that is in connection with the pile, a vibratory motion is communicated to the bar, very strong signs of an instantaneous current are obtained in the galvanometer, such as would have been obtained had the

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magnetism of the bar been increased; if, on the other hand, the vibratory movement is given to the bar very shortly after the current has ceased to pass, signs of an instantaneous current are obtained, as if the magnetism of the bar had been in some degree diminished. These phenomena are verified, whatever be the position of the bar in relation to the magnetic meridian, and they are altogether independent of the magnetism of the earth. These facts, in my opinion prove, and this has not hitherto been done, that whilst the bar is in a state of magnetization, any vibratory movements that are given to it, increase the susceptibility of the bar for magnetism, whilst the same action operates with a contrary tendency on a bar lately magnetized.

Finally, I endeavoured, but without success, to obtain the sound from rods of unmagnetic metal, when surrounded with the spiral traversed by the current.

From the sum of the observations made on the sound obtained by magnetizing and demagnetizing a bar of iron, and especially from the direction in which the vibration during the sound is made, I find that the most simple explanation that can be given, is to attribute it to a species of movement which the molecules of the bar or the wire must undoubtedly undergo from the middle toward their extremities, at the moment when the current passes through the spiral, and to a return movement of the same molecules at the cessation of the current. In fact, if filings are sprinkled on a sheet of paper, and the paper thus prepared is held over an electro-dynamic cylinder, at the instant when the current begins to pass, the filings will be moved, as if they were around the magnet. These movements suddenly cease, and the particles of filings return to their position as soon as the current ceases. Notwithstanding the cohesion of the particles of iron in the bar or wire, it is impossible not to admit that the electro-dynamic spiral, at the beginning and at the end of its actions, acts upon each of its particles as upon the filings.—[A.]

Pisa, January 20, 1845.

CVIII. *On the Vibratory Motions that are determined in Bodies, either by the Transmission of Electric Currents, or by their Exterior Influence*; with a note on *Ozone*. (Extract of a Letter from Prof. DE LA RIVE to M. ARAGO.) "Comptes Rendus," April 28, 1845. Vol. xx, No. 17, p. 1287.

(TRANSLATION.)

IN the sitting of March 21, 1844, of our Society of Phy-

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sics and Natural History, namely more than a year ago, I showed that a piece of soft iron, placed in the interior of a helix, gave out a very decided sound, when it was successively magnetized or demagnetized by the passage of a discontinuous electric current in the wire of the helix. This experiment, publicly made more than a year ago, and which I showed last summer to M. Becquérel, when he was at Geneva, has been also made in England by Mr. Marrian of Birmingham, as appears from the accounts given in the English Journals,* and in the *Institute* of the 8th of January last. In the sitting of our Society, on January 15, 1845, I likewise communicated certain experiments, from which it follows that a passage of a discontinuous current through an iron wire or a bar of iron in like manner gives rise to vibrations which produce a very loud sound. I added that this same effect is produced, but in a less degree, by the passage of a discontinuous current through all other metals. Now, I find, in the April number of the *Electrical Magazine*, which reached me yesterday, that Mr. Beatson of Rotherham, has observed an analogous fact;† but he contents himself with the bare mention of it, without entering into any detail. I have collected into a memoir, which is now in the press, the facts which I have just placed before you; as well as certain others relating to the magnetization of soft iron. Until this memoir appears, and in order to establish my right of priority, I have resolved to address to you these lines, which you will, I hope, receive with your usual kindness.

I arranged on a sounding-board, wires or rods of various metals, of different lengths and different diameters; the construction of the apparatus allowed me to stretch the wires more or less like the wire of a monochord. Each rod or each wire might be arranged so as to pass through the axis of a bobbin, surrounded with thick copper wire covered with silk, and rolled into a helix. The current, which was rendered discontinuous by means of a commutator, I made to pass sometimes through the wire itself, that was the subject of experiment, and at other times through the wire of the helix with which it was surrounded. The following are the results:—

With rods or wires of *iron*, the sound is nearly the same whether it arises from vibrations produced by the passages of the discontinuous current through the wire, or whether it arises from vibrations produced by the magnetization or demagnetization that results from the transmission of the discontinuous current through the wire of the helix. This fact would seem to

* Vide *Electrical Magazine*, vol. i. p. 527. † Ibid. 555.

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prove that the molecular arrangement or derangement resulting from magnetization is the same as that resulting from the transmission of the electric current through the iron; this analogy does not appear to me unimportant for the theory of magnetism. With regard to the sound itself, I cannot give a better idea of it than by comparing it to that produced by Savart's toothed wheel; it is a succession of noises resulting from the shock of metallic particles against each other, much rather than a musical sound. It is true that we hear musical sounds: but they are the harmonics of the sound, which the rod or wire would give out by the effect of transverse vibrations; they arise from the vibratory motion which the metal undergoes, but are not a direct effect of the electric influence to which it is subjected. We may, indeed, make them disappear by touching the vibratory body with the hand, without causing the fundamental noise to disappear.

When the iron wire is *annealed,* the sound which it produces by the passage of the electric current is much stronger than that which it gives out by the alternate magnetizing and demagnetizing action of the helix; the converse occurs when it is *hammer-hardened.* A *steel* wire gives out but a very feeble sound when traversed by the current, it gives out a much stronger sound under the influence of the current that traverses the wire of the helix. The sound given out by a well-annealed iron wire, when it transmits the current, is a very strong sound, greatly resembling the sound of church bells in the distance. It might be perhaps advantageously employed in the electric telegraph.

The tone of the sound varies with the velocity with which the discontinuous currents succeed each other; when the succession is very rapid the sound very much resembles the noise which the wind makes when it blows strongly. This remark is equally applicable to the sound produced by both modes.

I subjected to the same double influence wires of *platinum, silver, copper, brass, argentane, lead, tin,* and *zinc.* They all produced appreciable sounds, but more or less intense, both when they were traversed by the current, and when they were subjected to the exterior action of the current of the helix. For each there was no sensible difference between the sound that it gave in one case, and that which it gave in the other.

A remarkable thing is, that copper, brass, platinum, and argentane do not give sounds of any intensity, except when they do not undergo any sensible tension. As soon as they are stretched a little, the intensity of the sound is reduced;

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and it becomes almost nothing when they are powerfully stretched; the reverse takes place in respect to lead, zinc, and tin.

The length of the wire has no influence over the tone of the sound; it influences its intensity in such sort that the weaker the current is, the less length must be given to the wire, in order that its sound may be sensible; at least in as far as relates to the sound resulting from the transmission of the electric current through the wire.

The sound which the different metals produce, when they transmit a discontinuous electric current, appears to be due to periodical molecular displacements, which, as it were, bring about a kind of rubbing of the particles against each other. In order to give rise to these vibrations,—which, by-the-by, are both visible to the eye, and sensible to the touch,—electric currents of great intensity are necessary; but it is not necessary that they should be derived from piles of high tension. Five large-sized elements of a Grove's battery have served me in most cases. The metals, which are the worst conductors, are they which have appeared to give the most pronounced effects. Thus after *iron*, which by far surpasses all, comes *platinum*.

In order that the effect may be pronounced, it is necessary that the current should meet with more resistance in the metallic conductor, which it is to put into vibration, than in all the rest of the circuit, in which the pile is included. By this we see that, of all the effects of the current, those to which the phenomena I have just described have the most relation are calorific effects. Might it not be that the general phenomenon which the passage of the electric current produces in all conducting bodies is a vibratory motion, and that these molecular vibrations themselves give rise, according to circumstances, to heat, chemical decompositions, and physiological effects?

Many years ago, I pointed out a phenomenon which is intimately connected with the production of vibrations by the electric current; it is the disaggregation and transport of particles occurring in the charcoal or metal points between which the electric current that produces a luminous arc, is passing.* Two years ago, I had the honour of drawing the attention of the Academy to the vibratory effects that are observed in this case, and of showing to some of the members, especially to M. Regnault, the experiment in which the sound resulting from these vibrations is perceived. I have further to remark, that the fact pointed out by M. Peltier,† and

* Vide *Proceed. Electrical Society*, p. 75.

† *Vide* a subsequent page of present number of *Electrical Magazine*.

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which I have also had occasion to observe, namely, that metal wires which have frequently been used to transmit electric currents become brittle and friable, finds its explanation in the existence of the vibratory motions, which the transmission of a current determines in these wires.

A very remarkable species of vibration is that which is obtained by making the discontinuous current pass through the copper wire covered with silk, which is wound in a helix around a glass bobbin or bottle. The sound in this case is of a much softer and a more metallic tone; and at the same time much graver than that produced by the influence of the current upon a wire of the same diameter, placed in the interior of the helix.

The vibratory motion which results from the successive magnetization and demagnetization of the soft iron may be manifested under very varied forms. I will cite, as one of the most curious cases, that wherein we place in the interior of a bobbin, or a bottle surrounded with a wire rolled into a helix, very small discs of very thin sheet-iron or very fine iron filings. When the discontinuous current traverses the wire of the helix, the discs are seen to be agitated and to revolve around each other in the most remarkable manner; the filings have the perfect appearance of being in ebulition; if the current is intense, they dart in the form of jets three or four centimetres in height, like so many small fountains. This motion of the filings is attended with a noise similar to that of a liquid that is boiling.

I have included in my memoir several other facts, relative to the magnetization of soft iron, which seem to me not easily reconcilable with the received ideas; but of which I will spare you the exposition. I will confine myself to pointing out to you one only—it is, that a very thin disc of plate iron is not attracted even at a distance of less than one *millimetre*, by any arc of the poles of a strong electro-magnet of soft iron, provided always that the electro-magnet be sensibly greater than that of the disc, and that the latter be so placed that its centre be on the direction of the axis of the electro-magnet.

I shall not terminate this description of some of the experiments that I have had the opportunity of making on the connection existing between the relative displacement of the particles of bodies, and the effects of electricity and magnetism, without recalling to mind that all these phenomena are related to that important branch of the sciences, to which the remarkable observations that you made twenty years ago, on the influence of motion in magnetic actions, gave rise. The discovery of magnetism, developed in all bodies by rotation,

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with which you have enriched physics, has opened a field altogether new, in which there is yet much to be explored; I hope to be able to continue occupying myself on this subject; and I should be very fortunate were I to succeed in throwing light upon any of the obscure points which this class of researches presents.

On Ozone.

P.S.—I profit by the opportunity of now writing to you to add a fact which M. Marignac and myself have just observed, in addition to those which he has communicated to the Academy, by means of M. Dumas.* It is in reference to that principle which Mr. Schoenbein has described under the name of *ozone*, and which he thinks to be an element of nitrogen. M. Marignac has shown that ozone may be produced without nitrogen; and he has succeeded in recognising that it cannot but be either a particular combination of oxygen and hydrogen, or oxygen in a certain state. This last opinion, towards which I have long inclined, has been confirmed by the following experiment, which we made together. We passed through a tube a current of perfectly pure and perfectly dried oxygen; then, by means of two platinum points, we transmitted through this oxygen a succession of electric sparks from an ordinary machine. The oxygen immediately manifested the properties of ozone, namely, it acquired that penetrating and nauseous odour by which it is characterized, and it strongly blued iodide of potassium, &c. Thus, to confine ourselves to the actual results alone of the experiment, ozone is derived from oxygen alone; and in order to have a manifestation of it, the most simple and direct means is to pass a succession of electric sparks through oxygen.—[A.]

CIX. *Atmospheric Apparatus at the University of Pisa; and Note on Electricity from the discharge of a Gun.* By Prof. Ch. Matteucci. "Il Cimento," Nov. and Dec. 1844, p. 482.

(EXTRACTS.)

(After making mention of his visit to the Observatory at Kew, with Mr. Ronalds and Mr. C. V. Walker, and describing the arrangements there,† he continues:—)

* Vide page 56 of present Number of *Electrical Magazine*.
† Vide *Electrical Magazine*, vol. i. p. 213.

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I agree with the proposition of having this same system of insulation adopted in the atmospheric electrometer recently erected in the Meteorological Observatory of this University. Not having succeeded in procuring in our glass-works a conical base of glass like that of the Observatory at Kew, I made some little variation in the system of insulation described. I fixed at right angles to the upper extremity of a wooden staff a large solid rod of glass, about two feet long; and, by means of a large vessel of varnished tin, fixed by a metal arm at a certain distance from the glass rod, I protected it from rain. At a few inches below the glass cylinder, is also fixed to the vertical wooden staff, and parallel to the said cylinder, a piece of wood of about half the length of the cylinder. At the end of this piece of wood is fixed a pulley by means of which I raise to a little beneath the glass cylinders, a lantern, in which an oil lamp is constantly burning. The construction of this lantern is very simple; it consists of a small oil vessel, such as is commonly used, contained in a long truncated tin cone, closed at the extremity, and which has a few small holes round the surface by the lamp, and many others higher up, in order to allow the current of hot air to escape, which current is directed against the middle of the glass cylinder. The tin cone is fixed by a bayonet joint to the oil vessel.

(*After again adverting to the arrangements at Kew, he continues:*—)

In our observatory, a metal ferrule is fixed to the extremity of the glass cylinder, and to this ferrule is attached a small pulley. A lantern, similar to that which is employed to warm the glass cylinder, is raised by means of a silk cord running over the pulley; there is also a copper wire, one end of which is soldered to the oil vessel, and the other terminates at the electroscope, by being introduced through a large hole, so as not to touch the walls of the observatory. By means of a small leaden weight attached to the cord, an equilibrium is provided against the weight of the lantern; by raising or removing the weight, the lanthern can descend.

(*Here follow other notices of Kew which we may omit.*)

As soon as the atmospheric electrometer was mounted in the Meteorological Observatory of this University, I wished to review a fact that I had already observed, and published many years ago. Whilst the instrument is in action, if a gun is fired in proximity to the flame on the upper part of the electroscope, a great increase is perceived in the indications of the instrument, which are always of positive electricity; and, as this observation was first made in serene

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weather, the effect of firing the gun was easily explained to me, by attributing it to the better conductibility of the column of hot air driven against the flame or the points, which thus as it were increased the action. By reviewing of late and varying the experiment, I am led to recognise that the explanation given by me, with so much readiness, was not indeed the true one; and that the fact is reduced to a development of electricity by friction.* And, in fact, if, instead of directing the fire against the lantern so as to strike it with the powder that has not been burned, the fire be so directed that the said powder shall be driven above the lantern without being able to touch it, then the effects of firing the gun are entirely wanting to the electroscope. And yet in this case the column of dry air would be the most favourably circumstanced for the explanation that I had given; it is also the case, that at however short a distance from the lantern we fire, provided no traces of powder are seen on the lantern, the effects are entirely wanting. It remained, therefore, to see whether the friction, which was the cause of the development of the electricity, resided in the barrel of the gun, against the sides of which the powder was rubbed, or whether the effects were due to the friction of the same powder upon the body, against which it was driven. I, therefore, insulated the gun perfectly by means of silk cords, and placed it in connection with a delicate electroscope; I repeated the experiment several times, loading both with powder alone, and also with powder and shot, and I never had any signs of electricity in the gun after the discharges.† The friction, therefore, which is the cause of the development of the electricity, resided in the body that was struck by the powder. The experiment that I am about to relate, in proof of this conclusion, is both simple and very satisfactory. I prepare a tin disc of the diameter of about eight inches; I suspend it with a silk cord, and place it in connection with the electroscope. I load the gun with a little powder only, and fire against the disc, at a few paces distant. I thus obtain, as usual, very great signs of positive electricity on the disc. This having been done, I construct, horizontally, a kind of trough of tin, also insulated, and about twenty inches long, and I place it thus at a few inches from the disc. I then direct the fire so that the powder which passes from the gun shall brush along the tin trough, and

* Vide Elice's Experiments, *Electrical Magazine*, vol. i. p. 43; and our own views of the cause in the *Cabinet Cyclopædia*, vol. cxxxi.; Electricity, vol. ii. p. 93.—*Ed. Elec. Mag.*

† Compare with Elice's Experiments.—Ed.

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afterwards arrive in contact with the disc; I constantly obtain very powerful signs of negative electricity on the disc. If, on the other hand, the trough is in communication with the electrometer, the signs of electricity are positive. The powder and the disc, or any other body that is struck, acquire contrary electricities; that is, the powder is electrified minus, and the body struck plus.—[A.]

C. MATTEUCCI.

CX. *On the Employment of the Earth as a Conductor for the Electric Telegraph.* (Letter from M. CH. MATTEUCCI to M. ARAGO.) "Comptes Rendus," May 12, 1845. Vol. xx. No. 19, p. 1431.

(TRANSLATION.)

I HOPE that you will read with some interest the new experiments that I have lately tried once again upon the employment of the earth as a telegraphic conductor. Perhaps this letter may reach you at the moment when you are executing your great experiments at Paris;* and just in time for making on a large scale what I have been compelled to do on a small. M. Mangrini's experiments appeared to me so extraordinary, and to lead to results so new to science, that I commenced by repeating these experiments, employing every possible care in the insulation of the wire. I again extended my copper wire, No. 8, of commerce, along one of the high roads in the park of the Grand Duke at Pisa. The stakes of dry wood, with which I sustain the wire, were entirely covered with six layers of turpentine varnish, and were left several days in the sun. These stakes are fixed in the ground at ten or twelve yards apart; care must be taken not to allow them to lie on the ground before erecting them. The length of the wire that I extended was a mile. Each stake was 5 feet high, the wire was raised at least $3\frac{1}{4}$ feet above the surface of the ground. It is necessary to go along the whole line before commencing the experiment, in order to be quite sure that the wire is not touched in any point by conducting bodies in communication with the earth. To make myself certain of the insulation, I made a first experiment by interrupting the wire near one of its extremities, and introducing a galvanometer and a pile of four Bunsen's elements, charged

* Vide Report of *French Academy*, May 12, in a subsequent page of the present Number of *Electrical Magazine*.

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with water slightly acidulated. For excess of precaution, the galvanometer and the pile rest upon a plate of glass covered with varnish. The galvanometer employed was one with a long wire, and a perfect astatic system. It is the galvanometer by Rumkorf* that I had employed in my experiments on animal electricity. The needle remained perfectly at zero. When operating in the air, it is necessary to shelter the galvanometer from the wind: and, in order to try the insulation of the wire so as not to shake the galvanometer, it is well to keep the galvanometer connected with the wire, and to introduce or remove the pile, by holding the last zinc with an insulating handle and introducing it into the liquid, and taking it out again. Furthermore, when a few experiments of this kind have been tried, we soon become accustomed to all the requisite precautions, in whatever way we may desire to vary them.

The insulation of my wire, therefore, was perfect. On touching it with hands quite dried, at two points between which the galvanometer and the pile were included, the needle was immediately driven to 90°. In one of my experiments, the stakes had inadvertently been thrown upon the ground, which was still slightly moist from the dew, before they were erected, and the wire was extended. I was no longer able to obtain insulation. After assuring myself of the insulation of my wire in the manner described, I removed the pile, leaving the galvanometer. To one end of the wire I affixed a plate of zinc, which I let down into the water of a pit, so as entirely to immerse it, supporting it with a cord. I repeat that, for excess of precaution, every thing was insulated—that is to say, the portion of wire which descended into the water did not touch the ground before reaching the water, and the zinc plate was supported by an insulating thread. We had thus a plate of zinc in the water, connected with a copper wire a mile long, and perfectly insulated from the ground. The galvanometer formed part of the conducting wire, and was placed at the end, which terminated in the plate. There are precautions to be observed, in order that the experiment be accurate. Thus, if the wire which communicates with the plate be already connected with the galvanometer, and if, by taking the other end of the wire with the hand, we touch the other extremity of the galvanometer, we obtain a current that lasts as long as that end of the wire is retained in the hand. In this manner the circuit is completed with the water, the earth, the body of the

* Vide *Electrical Magazine*, vol. i. p. 478.

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observer, the copper wire, the galvanometer, and the zinc plate. So also, no traces of current are obtained, when operating in the above manner, while holding the end of the long wire with an insulating handle. In place of the zinc plate, I successfully substituted one of iron, one of tin, and one of silver; but there was never any sensible trace of a current. It is, therefore, clearly proved that a copper wire, perfectly insulated from the ground, of one mile in length, terminated at one extremity in the air, and at the other by a plate of any metal plunged into the water of a well or a ditch, is never traversed by a current sensible to the very delicate galvanometer that I have described.*

I am ignorant of the modifications which may be made to this conclusion by employing a wire several miles in length; but it seems to me that they may be anticipated. In the preceding arrangement the current is immediately obtained, providing any body in communication with the ground touch the wire; we merely see that the intensity of the current varies with the conductibility of the body that touches the wire. I will here relate a result that appears to me of considerable importance for the guidance of the experimenter. I have already mentioned the case of the stakes which had been thrown upon the ground still covered with dew. On employing these stakes we had signs of current with the arrangement that I have described, namely, with the metal plate plunged into the water. In another experiment the insulation was perfect at the commencement; but rain having come, the insulation was no longer perfect, as might have been expected; and then the sign of current commenced and remained. Let us now see the direction of the current. When it is a plate of zinc, or iron, or tin, that is plunged into the water, the direction of the current is such that it enters into the galvanometer by the long wire—namely, it travels as it ought to travel, from the zinc to the water, to the earth, to the wire by the moistened stakes, like as it enters into the wire by the body of the observers, when the experiment is made with the insulated wire. By employing a plate of silver plunged into the water, the current has a contrary direction, namely, it enters into the galvanometer by the wire connected with the silver plate. It is clear that,

* We may here remind our readers, that in the *resumé* we gave in our last No. (p. 594), of M. Mangrini's experiments, we purposely passed over certain of them, because the anomalous results were so obviously due to defective insulation, that they could not for a moment be admitted. M. Mangrini thought he obtained results *before* the circuit was completed, whereas the circuit was completed through *defective insulation.*—*Editor Electrical Magazine.*

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in this case, it is the copper wire which is attacked by the water that moistens the stakes, and which water is continued from thence to the soil, to the water, and to the plate of silver. This experiment may be very readily made when the wire is perfectly insulated. Let a plate of silver or of platinum be plunged into the water, and let the wire be touched with the hands moistened either with pure water or with water slightly salted, and the current will be directed, as before, from the copper wire to the observer, to the soil, to the water, and to the silver plate. All these results are perfectly simple. They are moreover readily anticipated, and they accord with our previous knowledge. Finally, the following is the result when a plate of copper is plunged into the water: whether with the wet stakes, or by touching the wire with the hand, the signs of the current are very feeble; but we can say that the direction of the current is always as if the wire, instead of the plate, were attacked by the liquid. On operating with the wire perfectly insulated, and on touching it with the hand slightly moistened with acidulated water, the current is strong, and is directed from the wire to the hand, as ought to be the case. In order to explain the case of the wet stakes, we must admit that there is more surface of copper in contact with the liquid in the sum of the points of wire that touch the stakes, than there is in the plate which is plunged into the water; which is very possible in my case, for all my plates had a surface of only about $1\frac{1}{2}$ ft. square.

We now come to another very simple experiment, and one which seems exceedingly important in electro-telegraphy. My wire, of about a mile in length, and perfectly insulated, is terminated by two tin plates immersed in the water of two trenches, which are distant about the length of the wire. The two trenches have no direct inter-communication. I had in the circuit the pile of four Bunsen's pairs, and Nobili's comparative galvanometer. In a first experiment, the pile and the galvanometer were side by side, at one extremity of the wire; in the second experiment the pile remained in its place, and the galvanometer was carried to the other end of the wire. In both cases the needle remained exactly at 27°, which proves the perfect insulation of the wire. I then had all the stakes removed, and the wire was extended its whole length along the ground covered with grass. The needle of the galvanometer still rested at 27°, as before, whether the galvanometer were on the side of the pile or at the other extremity. We therefore saw that the insulation was perfectly useless in this experiment, and that the current was

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transmitted in the same manner by the mixed circuit, whether the copper wire were insulated or not. We must not believe from this that things are the same in longer circuits, as I have already proved in my first experiments, and in localities in which the soil was moist.

Perhaps it might be sufficient for certain lengths, when we do not wish to insulate the wire, to employ one of larger diameter. One thing is certain, which results from my first experiments, from those made at Milan, from those lately made on a larger scale in England, at a distance of 88 miles, with several* miles of earth, and from the last experiment that I have just related—that the resistance of a mixed circuit, wire and earth, is less than that of a circuit of the same length entirely of copper wire, in making a series of experiments in which very great and variable lengths of earth are introduced. This does not prevent its being possible to succeed in finding the resistance of the earth, which I had not found when operating on lengths that have never surpassed $1\frac{1}{4}$ mile. M. Mangrini, when operating on lengths of several miles, found this resistance; and he gives, for the resistance of 1 kilometre, (·6213 miles) of earth, 273 metres ($298\frac{1}{2}$ yards) of his copper wire; but it would be desirable that this experiment should be repeated, and that the conclusions should be deduced from greater differences in the deviation of the galvanometer needle. I find, for instance, the following experiments described in M. Mangrini's Italian Memoir. His circuit was composed of 4 k. m. of wire, and 1 k. m. of earth; and then of 2, 3, and lastly, 4 k. m. of earth. The mean deviations, which he records in his Memoir are the following: $22\frac{5}{8}$, $21\frac{1}{16}$, 21, and 20. I do not think that, in reading the galvanometer, we can answer for a fraction of a degree, when the needle, even on an ordinary compass card, has a greater diameter than the interval of one degree. I therefore, until new experiments shall have made me change my opinion, continue to regard the resistance of the earth to the electric current as nothing, or almost nothing, except that which is met with at the first passage or change of conductors, which is constant, whatever be the distance between the two pits. The singular result to which I arrived last year, namely, that in a mixed circuit, wire and earth, in which there is at least $1\frac{1}{4}$ mile of earth, the resistance would be less than that which is due to the copper wire alone, has been again verified in the same locality. I have found it, and I still find it so singular, that I invite further experiments on

* 88 miles.—*Ed. Elec. Mag.*

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this subject. It is very possible that the effect is due to a feeble current, which always remains, developed by the two extreme plates, and which circulates with that of the pile.

We now come to some experiments that I tried, in order to establish an electric telegraph between two points separated from each other by the sea, as for example, in the case with Dover and Calais. It is impossible even to imagine a copper wire extended in the air to this distance. I believe the thing possible, although immensely difficult, if the wire be allowed to pass through the water. I made across the Arno an experiment, in truth, comparatively, a very small one, but yet it is of the same nature. I selected two wells on the two sides of the Arno; a metal plate is plunged in the water of each well, and is connected with a copper wire which passes insulated from the soil to the point where it is plunged into the water. I remained on one of the banks with a galvanometer introduced into the wire, and I obtained a feeble current developed between the plate and the wire that was immersed in the river; I had the pile of four Bunsen's elements introduced in the wire at the opposite bank, and the needle was instantly driven to 90°. It is therefore evident that a part at least of the current circulates in the copper wire immersed in the river, and returns by the ground interposed between the two wells. In another experiment, a Faraday's pile of fifteen elements was immersed in the middle of the river, and its two extreme poles were connected by two wires with the plates that were immersed in the wells. When the pile is not immersed in the river, the galvanometer indicates a feeble current, the origin of which we well know.

When the pile is immersed, the needle deviates to 90°, and the circuit is still completed by the earth comprised between the two wells. If we would extend these results to the case of Dover and Calais, enormous difficulties present themselves; but the two great nations who are separated by the British Channel well know how to conquer difficulties. I ought also to add, that my experiments were made under very unfavourable conditions; and first amongst these, I may place the direct communication between the water of my wells and that of the river.

Finally, I would communicate to you one other project which presents fewer difficulties than the preceding, but which has, at least in appearance, much fewer chances of success than the former. Imagine two metallic arcs, insulated from the ground, and the extremities of each of these arcs terminated by large plates of metal. The two nearer plates of these two arcs are immersed in the same liquid, which may be the

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water of a basin, a river, or the sea; the other two plates are immersed in the water of two wells, or are buried in the ground: finally, one of the arcs is interrupted by the pile, and the other by a galvanometer, which form a part of it. I made the experiment in a basin 10 feet in diameter, and the experiment succeeded; namely, on closing the circuit of the pile, the needle deviated to 90°, and the circuit was evidently made through the water of the basin on one hand, and the soil on the other. Evidently, in order that this should succeed, we must imagine that, between the two plates of the same arc, interrupted by the pile, the resistance is greater than that of all the bed of earth which separates the two piles. This was readily the case with my basin, which was not deep, and was built of brick. The experiment failed when, instead of the basin, I interposed the river Arno. Shall we conclude from this that it would be impossible that it should succeed across the sea? I dare not be absolutely sure of this. Imagine, then, two wells, at a very great distance from the seashore, even separated from its coasts by a chain of mountains, if such were possible; make these wells very deep, surround them to the bottom with bricks that are almost insulators; look at the good conductibility of the water of the sea, and if I do not at this moment create to myself an enormous illusion, it seems that I am permitted to preserve some hope.

It is only on account of the vastness of the result that would be obtained, that I am confident of being excused by you and by the learned world of having made public a project which has many appearances of a dream, but which I desire with all my heart to see realized.—[A.]

SCIENTIFIC MEETINGS.

ROYAL INSTITUTION.

May 2nd, 1845.—*On the Applications of Electro-Metallurgy, by* Mr. James Napier.—Having described the nature of voltaic pairs, and the order in which the electrolytic action is determined, Mr. Napier alluded to electro-cloth as one of the applications of this art. The troughs, in which the sulphate of copper for this operation is contained, are four or five feet deep; the cloth is made conducteous by plumbago, or otherwise; the zinc of the generating cell is excited with a solution of two or three pounds of sulphate of zinc to a gallon of water, which keeps the action moderate and

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saves mercury: when the action has fallen, one-third of the zinc solution is removed, and its place supplied with water. If means are not taken to keep the solution of copper uniform, either by moving the cloth or the solution, one part of the deposit is destroyed by cross actions. In respect to the cause of voltaic reduction of metal, Mr. Napier did not rely on the molecular theory; he said that no base, or base radicle, travels in the electrolyte, but that the solvent itself travels: he explained his view by reference to a diagram.

He now described the process adopted in Elkington's manufactory for preparing the argento- and auro- cyanide solutions. The best yellow prussiate of potash is well dried upon an iron plate, and is then reduced to fine powder; carbonate of potash is similarly treated; eight parts of the former are well mixed with three of the latter. They are placed in a hot iron pot on the fire, and when melted are covered and allowed to remain for about half an hour; the contents are then poured upon an iron plate, and form the simple cyanide of potassium. Nitrate of silver is next prepared, and is precipitated in the form of cyanide of silver by the careful addition of cyanide of potassium: it is well washed, and is now dissolved in cyanide of potassium to form the argento-cyanide. The articles to be plated are first boiled in potash, then scoured with fine sand; and afterwards passed through nitric acid, and washed in boiling water. After a few seconds of electric action, they are brushed with the scratch brush to perfect the cleansing, and are then replaced to complete the plating, which process is accomplished in five or six hours; after which they are scratched and burnished. The auro-cyanide is not so easy of preparation. The plan formerly adopted of dissolving oxide of gold in cyanide of potassium is not adopted now: the solution is prepared by electrolysis; a porous tube, containing cyanide of potassium in solution, is placed within a vessel of a similar solution; within the tube is a gold anode, within the vessel the cathode is placed; the liquid is electrolyzed, and the gold being dissolved forms the gilding solution, which is removed from the porous cell for use; when sufficiently saturated the liquid in the outer vessel becomes a solution of potash. Electro-gilding is conducted with a solution at the temperature of 123°. The operations of gilding and plating were shown: the mode of obtaining solid silver copies of articles much under-cut was then given. To twelve parts of glue, carefully melted, is added three parts of treacle, and the whole is well incorporated; an elastic mould of the object is taken; the object being removed a cast is made in the mould with a composition consisting of three pounds of tallow and wax, and a half-pound of rosin. To this composition the power of reducing silver is given by mixing with it some phosphorus dissolved in bisulphuret of carbon; it is removed from the mould and dipped into nitrate of silver; and to ensure success is then dipped into chloride of gold, by which its surface becomes covered

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with a metallic film. A certain substance of copper is now deposited upon it, and the composition is melted away. The inside of the copper mould is well cleaned, and its outside is protected with a mixture of tallow and pitch; when it is placed in the silver solution and subjected to the action of the voltaic current until a sufficient substance of metal is deposited. The copper is now dissolved off with a solution of perchloride of iron. The last application of the art was the separation of metal from the ore. A quantity of native sulphuret of copper for instance, has been taken and roasted in the usual way; it has been placed in a furnace in an iron pot, and lime and soda have been added as fluxes, so that the whole should enter into the fluid state which is so essential to electrolysis. The pot is now made the cathode of a voltaic arrangement, and a plate of iron, as an anode, is immersed in the fluid mass; in the course of an hour, a solid mass of copper appeared at the bottom of the pot, while other pots, similarly charged, and under the same circumstances, except that they were not acted on by an electric current, had given up no metal. Mr. Napier stated, that from twelve to sixteen times more copper was deposited than the equivalent of battery action could account for. The process is patented.

May 16.—Rev. E. SYDNEY "*On the Electricity of Plants in the several stages of their Development.*"—That a few vague ideas have been long afloat in reference to some relation, which may exist between electricity and vegetation is well known; but nothing definite has been put forth. A few isolated experiments have from time to time been made; but here the matter has rested. Mr. Sydney would not, therefore, attempt to appear before the members of the Royal Institution with any loose and ill-digested theories,—theories founded on elements so inefficient, and so incomplete in themselves; his sole object was to appear as an humble inquirer after truth, to take a hasty glance at the few facts of which we are in possession, and so to put the case as to induce philosophers and others, who have time and means at their command, to turn their attention to this somewhat neglected field of research. He advanced five propositions, the spirit of which was as follows:—1st. *Electricity* APPEARS *to exercise an influence on the growth of plants.* He laid great stress on the word *appears;* he would not, in the present state of things, say it actually *does* exercise an influence; but would merely allude to certain facts, making no comment upon them, in which this influence seems to be *probable.* He here referred to some myrtle trees, at Edinburgh, which, in 1746, when subjected to electric influence, increased in vigour;—to seeds electrified with advantage by the Abbé Nollet;—to plants so treated, with like results, by Bertholet,—and to experiments by Jalibert and Pouillet, in which plants were made negative and the air positive; at the same time he alluded to experiments, by Ingenhouz, in which negative results were obtained. He also quoted Mr. Pine's statement, which we give in full:—

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"A medical electrician residing in Maidstone, whose powerful machine was in frequent action during the day, assured me of the following facts:—A narcissus plant which, from previous neglect, was in a languishing state, having been placed in his laboratory, soon began to exhibit signs of extraordinary vigour; it grew to the height of thirty-six inches, or about thrice its ordinary dimensions; flowers retained their colours while the seeds were forming, and at last dropped off without assuming a withered appearance; a turk's cap lily drooped during the hours of the night, and resumed its vigour with the returning action of the machine."*

He alluded also to other cases of the favourable growth of electrified seeds, and to Davy's opinion in favour of the influence of electricity. To these he added an experiment of his own, in which an insulated and electrized hyacinth gained in vigour over one growing under ordinary circumstances. The insulation was such that he has taken a spark from the plant forty-eight hours after the charge had been given. He would draw no hasty conclusions from these facts; but would simply advance them in support of his proposition of the *apparent* influence of electricity over vegetable growth.—2d. His next point was, that *the conducting energy of the fluids contained in vegetable tissues is potent;* in illustration of which he discharged the prime conductor of the large machine by approximating blades of grass, growing plants, and so on; we did not quite catch his views on this matter, but we think to have heard him say, that he did not for a moment suppose vegetable tissues to be better conductors than metal wires, as had been asserted by Mr. Pine. As the experiments were not comparative, but very general, they may be presumed to indicate that vegetable points, like other points, draw off electricity; and that the fluids in vegetable tissues, like other fluids, conduct electricity of high tension. He showed a staff of dry wood, which had been rent by lightning during a hail-storm in Norfolk, in proof that the moist parts and not the dry, conduct. He also alluded to the impossibility of passing an electric shock through a circle of individuals standing on a plot of grass. He mentioned the experiment described by Mr. Pine, in which it had been shown that

"A coated jar, having forty-six inches of metallic surface, was repeatedly discharged by the activity of a vegetable point in four minutes six seconds; while the same jar, charged to the same degree, required eleven minutes eighteen seconds to free it of its electrical contents, by means of a metallic point: the points in both cases being equi-distant."†

On this head, he pointed to a diagram of Mr. Weekes's gold-leaf electroscope, fitted up with a branch of *butcher's broom*, instead of the usual metallic point. Mr. Weekes's account of this instrument, in a letter to Mr. Pine, is as follows:—

"Armed with your *vegetable detectors*, it has shown symptoms of electricity by the passing of clouds at a great altitude, and under various other circumstances, in which electrometers with metallic points, placed by its side, gave no indica-

* Vide *Proceed. Electrical Society*, p. 164.
† Vide *Proceed. Electrical Society*, p. 221.

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tions whatever. This appears to me so decided a proof of the superiority of vegetable conductors, that it admits of no contradiction."*

After giving a few other facts, he passed on to the third proposition, which was,—3rd. That *there are indications of adaptation in the form of plants, in their various states of development, to electric influence.* The pointed nature of plants in their growing period, as contrasted with the comparative loss of points in the fruiting season was here put forth; and it was also stated that too great a stimulus is most unfavourable to fruiting; as, for instance, a pear tree ceases to bear in a tropical clime; and it would appear that the points, by which the electrical stimulus is conveyed, are removed when the stimulus would seem to be hurtful. 4th. *Are there not some natural phenomena that confirm our views?* Of these phenomena we have the fact, that the vine and hop plants grew very vigorously after thunder-storms; and Mr. Sydney cannot admit the popular belief that, in the latter instance, it is due to the destruction of the aphides by the storm; he does not allow that the aphides are so easily destroyed. It is also a common remark with gardeners that peas pod well after thunder. He quoted the dryness of the air, and hence its insulating power during the spring months, when it is presumed that there is greatest need of electric stimulus, as a further illustration; a dryness, the value of which is recognised in the well-known proverb, "a bushel of March dust is worth a king's ransom." He spoke also of the influence of electricity in assisting fluids in their passage through capillary tubes. He also mentioned the influence of palm-trees in collecting moisture from the air; and on giving Mr. White of Selborne's observation that trees were, as it were, "perfect alembics," he quoted our own notice of an illustration of this, that occurred at Streatham, March 19th, 1843.

"This phenomenon so peculiarly presented itself to my notice during the morning and the evening of the 19th of this month, a day characterized by much fog, which had been preceded by several cloudless and most genial spring-days, that it will not be out of place in me to say how much indication there was of a power in action, beyond what is commonly assigned in the production of such effects. The roads were dry, and, in places, dusty; but the ground around trees, especially those of large size, was literally deluged with water, quite as wet as if a heavy summer shower had passed. I could not attribute this to mere condensation; for it did not occur in such degrees in contact with fences and buildings; &c. &c." †

To this, he added observations on simooms; saying that there are no plants where simooms (which appear to result from a highly electrical state of the atmosphere) occur; and he gave Brydone's remarks of the different characteristics of dry soil, and soil covered with vegetation, as noticed by himself on Mount Etna; this he endeavoured to illustrate by the different effects on the charged conductor of a piece of bare chalk, and of a piece covered with moss. The 5th proposition was, *Do not the forms and geographical distribution of plants exercise an influence over their electrical pro-*

* *Vide* STURGEON'S *Annals of Electricity*, vol. iv. p. 247.

† Vide *Proceed. Electrical Society*, p. 559.

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perties? In answer to which question, he took a hasty glance at the luxuriant foliage of the tropics; and at the evergreens, with their infinity of points, of the colder regions, as compared with the deciduous trees, and their more rounded leaves, of our more temperate climes. He here also alluded to Williams's *Climate of Great Britain*, a book that is little known, and is now very scarce,* and which contains several very peculiar ideas in reference to electro-vegetation; and also a strange proposition for electrizing the atmosphere of Great Britain. This proposition, to which Mr. Sydney briefly reverted, we give entire for the benefit of our readers:—

"Suppose, therefore, a building erected, and furnished with machinery something similar to a cotton or silk mill; and that the various movements consisted of cylinders or plates of glass, fitted up with rubbers, &c. for exciting electricity; and so arranged as to convey the electric matter into an insulated upright bar, terminating without the roof of the building, in a large lamp or a series of lamps and points for again diffusing the electrical matter in the circumambient air? I find, by calculation, that a force adequate to work a common pair of mill-stones would give motion to twelve hundred such electrical cylinders or plates of glass. . . . . Might not one or two buildings of the nature I have described, furnished with the requisite apparatus in each county, be adequate to effect all we want, so as to render the seasons more propitious to the health of our growing crops. . . . . Such powerful machines, as I have described, might perhaps occasion local accumulations of electric matter, and thus excite frequent thunder-storms; if so, a greater number of smaller exciting instruments might be applied in different parts of the country," *vide* p. 349.

He next mentioned the recent experiments by Mr. Forster, at Findrossie, in Morayshire, at which we hinted in our last, and of which we may now give a more detailed notice; and, as these experiments have been very much alluded to lately, we will give them in the words of the chairman, from a statement read before the *Tring Agricultural Association*, in the county of Hertford, on October 18th, 1844; which statement reached us in a circular letter, proposing the formation of an *Electro-Cultural Society*:—

"Mr. Forster, bearing all these, and many other electrical facts in mind, resolved to place two poles, four feet high, in his front lawn, which had recently been laid down with chevalier barley and grass (after draining and subsoil ploughing it); and over these poles, which he fixed due north and south of each other, he stretched a common piece of iron wire, fixing the two ends of it to stout wooden pins, driven in close to the earth, and on the edge of the plot of eight English poles; and around the edges (which were straight lines) he sunk, about two or three inches beneath the earth, two wires of equal length, the ends of which were fixed, and in contact with the two ends of the suspended wire. And this wire must not be too tight, for its contraction in cold nights would break it in two, or pull away the fixtures, and thus defeat the object in view."

Afterwards, having learned the power of vegetable points in drawing off electricity, he feared that his poles were too short; and

"That when the points of the barley plants should reach one foot high, all the electricity that the suspended wire might before that have collected, and conveyed through the buried wire to the roots of the plants, would be abstracted by

* *The Climate of Great Britain, or remarks on the changes it has undergone, &c.* By John Williams, Esq. Baldwin, 1806. 8vo. pp. 358.

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the points of the barley; and thus the suspended wire getting nothing from the air, could not, of course, supply any thing, so that all the additional electrical influence would cease. Mr. Forster, therefore, next day placed poles eleven feet high, with wires, &c., exactly the same, except that the space surrounded by the buried wire was twenty-four poles, English measure.

"All the results are yet (October 1st) imperfectly known, but these are, or were, evident:—

1st.—The barley plants on the two small plots (of eight poles each) soon became darker in colour, and grew faster, until they had attained to about a foot in height; the darker green colour then gradually disappeared, and at the end of a fortnight after there was no perceptible difference but in the height of the young barley plants; and even this ceased to be very apparent as the crop advanced.

2nd.—When the barley of the larger, or twenty-four poles' plot, was six inches high, it assumed the same lively dark green, and grew faster than the surrounding unelectrified barley-plant,—and this difference it maintained up to the last, except that the colour, of course, in time become yellow; and it was curious that this change occurred later than in the rest of the crop. The number of stooks was also greater, and each larger when reaped; the ears from one grain of seed more numerous, and longer. The corn also is larger and harder.

"..... To make assurance doubly sure, Mr. Forster fixed to the short four feet poles of one of the smaller plots, pieces of dry pine wood, eight feet high; and suspended two wires to them; one at that elevation, and another a foot lower down, and was pleased to find that, after some time, this plot *partially resumed its former darker green colour.*"

In a letter, dated November 29th, 1844, Mr. Foster writes:—

"I can only *now* give you the good news, that the day before yesterday I thrashed, weighed, and measured the electro-cultured chevalier barley. The space it occupied was twenty-three English poles (more than one pole of the twenty-four pole electrified spot having been occupied by Victoria barley). The product was—from the twenty-three poles—fifteen bushels of barley; the bushel weighing 54¼ lbs. (2¼ lbs. beyond the usual average), thus giving thirteen quarters or 104 bushels per acre. The weight of the straw from the twenty-three poles was 1350 lbs. equal to 9390 lbs. per acre. I do not know the usual average of barley straw per acre, but it is easily to be ascertained and compared with this."*

After mentioning the experiments, of which the above is an authentic record, Mr. Sydney described certain arrangements made by Professor Ed. Solly, in the Horticultural Gardens at Chiswick: in one case, wires are stretched from poles, from which are suspended other wires *pointing* to the crop; in another case, wires are pointing upwards from the crop; other arrangements also appear to be made. Mr. Sydney now alluded to galvanic arrangements; and, in some that he had himself conducted, there were undoubted evidences of success. He buried plates, so that the current developed should pass among the crop. Specimens of potato and other plants were on the table, in which the contrast between the normal and the galvanized plant was unequivocal. He mentioned pines, mustard, cress, cineraria, fuschia, &c., as having been successfully operated upon. The action killed a geranium; and balsams were destroyed.

* "It appears, from *Stephens' Book of the Farm* (see p. 1070), that, of a very large crop of barley of sixty bushels, the straw weighed 3080 lbs. per acre; so that, instead of double (as Mr. Foster had thought his crop to be), the straw was more than triple the average."

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In conclusion, he begged it to be understood that his main object in putting forth these several facts was, that the attention of philosophers might be drawn towards the subject; and that so the true relations between electricity and vegetation might be determined; and the best mode of associating them might be discovered. So far as our knowledge at present extends, Mr. Sydney believes that electricity may be advantageously applied in horticulture. The agricultural question remains to be solved; the data are at present so scanty, and the facts so disconnected, that nothing but experiment can serve our purpose: when more experiments have been made—and many are now in progress—the philosopher will have material on which he may be able to work, and a solution to the question can then alone be expected.

LONDON INSTITUTION.

April 9.—This was the fourth and last *soirée* of the season. The Rev. Dr. Scoresby delivered a lecture, and gave *Illustrations of Magnetic Principles and Phenomena.* He began by calling attention to the comparatively modern date of all our true knowledge of the principles and relations of magnetism; and he reverted to the not far distant period when magnetism was known only in its application to the mariner's compass, and to the construction of a few toys. But when Œrsted had put forth his remarkable discovery of the relations subsisting between electricity and magnetism, a new era was opened to the philosopher. After dilating, in the spirit of a Christian philosopher, on the wonderful mechanism of the universe, he pointed out the remarkable fact that the connecting link, by which the whole is held together,—that the forces by which it is retained in its integrity, are all intangible. They are not manifest to our senses, but they are evident to the mental eye by their effects. Had such been the will of our All-wise Creator, we might have been endowed with fifty senses, instead of five; and have had powers given to us by which we could have obtained a direct view of the mysterious imponderable agents known to us by the terms light, heat, electricity, and magnetism. Such powers having been withheld from us, undoubtedly for wise purposes, we are only able to obtain partial glimpses of these intangibles, and to argue from things seen to those which are veiled from our view. The identity of electricity and magnetism has been long a favourite theme with philosophers; but there appears to Dr. Scoresby *primâ facie* evidence that they cannot be identical, because they are mutually convertible,—one is at right angles, or is tangential to the other. They are, it is true, always so far co-existent, that wherever one is, the essence of the other is present; but this other, as Mr. Grove would express it, is correlative; it may be *educed* from the first, but it *is* not the first itself. Besides, both cannot be primary agents; one presupposes the other, or *vice versâ*. It would seem, however, that

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these forces are proximate to the Divine hand; that they are the messengers by which Divine Providence is brought into action for the sustentation of the universe.

Descending now from these higher and loftier thoughts to our own more peculiar intercourse with matter and motion, Dr. Scoresby pointed out the singular property possessed by ferruginous bodies of evidencing magnetic forces; so much so that iron may be in a manner regarded as though it were to us an auxiliary sense. The first property of magnetism, to which he adverted, was *attraction;* in illustration of which he gave the usual experiments. When explaining the induction of polarity which precedes attraction, he gave a beautiful experiment; he applied a bar of very hard steel, that was free of magnetism, to a magnet of moderate power, without their evincing any mutual attraction; but when the bar was applied to his powerful magnet, built up of thin plates of steel, it was violently attracted. In the former case, the weak magnet had not power to disturb the arrangement of the particles of steel of so hard a character, and to polarize them; while in the latter this was readily accomplished. Alluding to the mutual relation between magnets and electric currents, he said that if magnetic tractors had any influence, for good or for evil, over the human frame, this influence would have a much greater chance of being exerted, if the human body were made part of a galvanic circuit. He left these matters, however, to the initiated. Among the applications of magnetic powers, he mentioned the mask of magnets, which had been introduced (among the needle-grinders especially) at Sheffield; by means of which the minute particles of steel were arrested ere they reached the lungs. In the cases where workmen have been induced to adopt this sanitory precaution, the mask is found fringed with fine particles of steel after the day's work; all or most of which would have entered the lungs, but for this provision. The attractive power of magnets is also employed for abstracting the filings of iron from among the dust of other metals of a more valuable character.* This was illustrated by separating iron nails from others of a different metal. A case was given in which a nail had been left on the floor of a large room near the centre, under the carpet; instead of removing the carpet, a magnet was fixed to a rod, and passed under the carpet to the spot where the intruder lay, and the evil was soon removed.—*Polarity* was the next point. We took the hint of an extemporaneous pivot for a needle, seeing that Dr. Scoresby inverted a precipitating glass on a point, and laid the magnetized bar on this: it rotated very readily. He spoke of the fact that one magnetic pole cannot exist without the other; and gave the experiment of breaking a

* This reminds us that magnets have been used in paper-mills to abstract from the pulp the little particles of iron, which arise from the abrasion of the machinery, and which, under the form of peroxide, frequently disfigure common papers.—*Ed. Elec. Mag.*

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magnetized bar; and impressed upon his hearers the fact that in magnetizing a bar, nothing is added to the bar, nothing is introduced into it, but that a development of powers inherent in iron is produced, that a molecular disturbance and re-arrangement takes place; and that while the magnetized bar receives nothing that it had not in itself before, the magnetizing bar loses nothing;—at least loses nothing so far as its action on the other bar is concerned.—*Direction* was the next point that presented itself. The mode by which the direction of a needle is determined by the resultant of all the magnetic forces acting upon it, was shown by placing a mobile needle over a single bar magnet; and then in the vicinity of two magnets. The latter case was given as a rough illustration of the action of the earth, which must be regarded as if possessed of at least four polarities. The direction of a needle obeys the same laws, whether the forces act on it merely through an interval of air, or through rock, iron, or other materials. This property is applicable in many cases; as, for instance, Dr. Scoresby alluded to the railway tunnel beneath Liverpool. He stated that the men were tunnelling towards each other, and being under the erroneous impression that the works were several feet apart, they blasted in one tunnel, and blew in the rock and debris upon the men in the other. But by means of a compass needle in one tunnel and a powerful magnet (whose action on the needle had been predetermined) in the other, Dr. Scoresby could have pointed out the exact thickness of the rock. The magnet he would use was on the table; a wooden base board supported two upright pillars; these sustained a wooden bar, moving in a horizontal axis; through this bar, and at right angles of it, were the magnets; they consisted of two bundles of hard, thin steel plates, magnetised and assorted according to the plans described in our extract from Dr. Scoresby's work on Magnetic Investigations.* The magnets were held in their position by wooden screws; and could be placed with the poles coincident in direction or opposed to each other; in the latter case, a bar placed across the poles at one end converted the system into a horse-shoe magnet. The mode of using this arrangement was shown by means of a distant compass needle placed on the table; when the magnets were in a vertical direction, the sum of the forces produced equilibrium; but when the axis was turned, the distant needle was deviated. Dr. Scoresby said, that by such means, he could determine the thickness of two or three feet of rock to a quarter of an inch; and of ten feet to two or three inches, by means of tables previously obtained by experiment; he could operate through distances of 100 or 150 feet. On hearing this at Liverpool, an engineer had expressed his regret at not having known of it some years ago; for he had been carrying two tunnels in a mine beneath the Frith of Forth; and, through want of some more accurate means than he had employed for determining the distance between them, the men run one tunnel into the other, and

* Vide *Elec. Mag.* Vol. i. No. 3, p. 222.

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the works fell in. It had reasonably occurred to some that iron or iron-stone might interfere with the results: this, however, is not the case; for on placing the magnets and the needle, one on each side of a locomotive engine, the effect was not interfered with. An amusing incident occurred, while these results were being produced:—two men, evidently of an observant turn of mind, were looking on; and, when the action was made to occur through the engine, one of them remarked to his companion:—"Why, Tom, what do you think; he sees through it?" Most surely this all-penetrating force of nature does indeed feel its way through stone walls, and gates of brass and iron. The only variation that occurs in the case of iron is that as the magnet induces magnetism in the iron, the deviation of the needle increases; because it is acted upon by the sum of the two forces.—And this observation naturally led to the fourth property of magnets, namely, *induction.* Under this head, Dr. Scoresby showed the mode of magnetization first by contact; and stated that what little loss of power is experienced by the inducing magnet, is due, not to abstraction of power, but to the resistance of the ferruginous bodies, on which it may have to act; a resistance that reacts in its full force (which, however, is not great) upon the inducing magnet. The difference between the permanent magnetic properties of steel and the transient properties of iron was next shown; and also the relations between similar and dissimilar poles. The lifting power of a certain bar of magnetized steel was shown by suspending to it the greatest weight it could sustain; when an iron bar was laid upon the steel bar, the weight fell. The rationale was explained:—the marked pole of the steel bar induces a converse polarity in the end of the iron bar that is in juxtaposition to it; and, consequently, the lifting power of the system is equal to the power of a pole of the steel bar, *minus* the power of the contiguous pole of the iron bar; and the weight falls. The induction of the large magnet on soft iron *not* in contact was very great; a large key, held at five or six inches distance, acquired enough magnetism to raise two other heavy keys, one on the end of the other. An ingenious illustration of induction was given, by holding one end of a vertical key *in front* of the pole of a magnet lying horizontally; under these circumstances, the key had considerable lifting power; but, as it was gradually raised, still in a vertical direction, in front of the magnet's pole, the distribution of the reduced magnetism gradually changed; and the weight dropped off. In like manner, a key placed vertically *above* a pole of the horizontal magnet acquired sufficient magnetism to remain suspended to another key, applied above it; but, in proportion as it was carried onwards, in a horizontal direction, toward the centre of the magnet, the force diminished, and the key fell. The fable of Mahomet's coffin is shown by allowing the attractive power of the large magnets to act upon an iron model held down from actual contact by a thread. The power of the system of magnets was shown by applying to

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them a mass of small nails; they took up about 10,000; and are able to sustain 12,000. Under these circumstances, the nails acquire a peculiar adhesive power; they have a kind of paste-like feel, and can be moulded into various forms.

Magnetic induction by means of the earth, and percussion as a constraining power, was next introduced; the magnetic equator, or position of no induction, being a plane at right angles to the direction of the dipping needle, was explained. Dr. Scoresby here gave a very ready means of obtaining a magnetic needle:—take an ordinary steel knitting needle, place it between the wards of a key and break off a portion; next take the *kitchen* poker, not that of the drawing-room, for the latter is case-hardened and resists feeble magnetic action, and place the piece of knitting needle vertically on the poker, and give the latter and the needle two or three blows with a hammer: then poise the needle on the glass of a pocket-watch, and it will evince considerable directive power, placing itself in the magnetic meridian. He here mentioned a magnetizing experiment, which he conducted at about 600 miles from the north pole: he began with six small steel bars; he magnetized two of them by percussion in the position of the dip; and by parallelogrammation and interchanging, he soon produced magnetism in a horse-shoe, capable of sustaining sixty pounds. He here mentioned, in reference to his plan of building up magnets of thin hard plates that each of the two he had employed in the lecture contained ninety-six plates; that they were not, as in the old plan, merely tempered at the ends, but were tempered throughout in hot linseed oil, at the temperature of 500°. He showed us three compass needles, two from her Majesty's stores, and one of his own; he magnetized each by a similar application to the large magnet, and tried respectively the sustaining power of each; his own was much superior. He then placed each on a *test magnet*, to examine their relative tenacity; and, while the other two lost all their power, his own was nearly as active as before. He adverted to the great want of care that had prevailed in the construction of mariners' compasses for the navy; and alluded to one case, in which his brother had nearly run his ship upon a shoal through the inactivity of the needle, and to another case, in which the needle was so sluggish, that an officer would stand by and give it taps, in order to make it move; he hoped that the improvement of later years would put an end to these unpardonable negligences; and that ships would never more have needles that scarcely know which is north and which south. Time, which will not wait either for friend or foe, compelled him now to conclude, long, very long, before his subject was exhausted.

In the library of the institution, among the subjects, were specimens of brass reduced by the voltaic current, and also a large electrotype ($2\frac{1}{2} \times 2$ ft.) of a portion of Thorwalsden's Triumph of Alexander, which we had ourself the pleasure of exhibiting. There

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were also some powerful electro-magnetic and other electrical apparatus, by Mr. Hearder of Plymouth, which we will describe hereafter.

SOCIETY OF ARTS.

Jan. 28.—A *conversazione* was given; among the objects exhibited was WHEATSTONE and COOKE'S *Electro-Magnetic Telegraph*, constructed for the South-Western Railway.

April 23.—A *portable Electrical Machine*, by CHARLES VARLEY, jun., was described.—Two glass tubes are arranged, so that one passes within the other, the inner of the two being the longer (20½ in.) and having a handle of wood. The inner tube is the *electrical machine;* the outer is the *Leyden jar*. The outside coating is a brass jacket, commencing at a certain distance from one end of the tube, and terminating at the other end, where it bears a ring-formed rubber, furnished with the usual amalgam. By passing the inner rod backward and forward through this ring, it is excited. The inner coating of the outer tube is tin-foil, not reaching to the ends of the tube. This tin-foil receives the charge from the excited glass; a narrow slip of foil connects the inner lining with a brass ring at the free end of the outer tube; between this ring and the brass jacket the discharge is made. A smart shock may thus be obtained with about a score of passes.

May 17.—The secretary read a paper by Mr. JAMES NAPIER, *on the Reduction of Ores by Voltaic Electricity*. The facts contained in this paper are given to our readers in the report of Mr. Napier's lecture at the Royal Institution, May 2nd. (*Vide* p. 44.)

IMPERIAL ACADEMY OF SCIENCES AT ST. PETERSBURG.

1844. First Half-year.—*Magnetic Observations, &c., made in* 1841 *on the Shores of the White Sea and the Frozen Ocean.* By M. ALEXANDRE SAWELJEV.—The polar regions of European Russia have been little explored in respect to terrestrial magnetism. The only exact observations known to us, on this point, are those of dip and intensity, undertaken on several points of Lapland by MM. Lütke and Reinecke; and in Nova Zembla, by MM. Lütke, Pachtussov, and Liwolka. With regard to the eastern coast of the White Sea, the peninsula of Kanin, the island Kolgujov, and the Timanski coast of the small Samojedes, we only know certain points, where the declination of the needle had been observed by some naval officers, who were occupied in the hydrographic description of its coasts. Besides, it is proper to mention that the chart of the shores of the Frozen Ocean, and of Cape Kanin to the mouth of the Petschora, which was drawn out in 1826 by the pilot Bereshnych, has not yet been published.

In the summer of 1841, M. A. Saweljev made magnetic, as well as other observations, at ten different places:—1st, the town of

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Mesen; 2nd, Cape Kanin-hoss; 3rd, isle of Kolgujev, at the mouth of the river Waskina; 4th, Czernoikoss, at the mouth of the river Indega; 5th, the mouth of the river Shemtschusnaja; 6th, Cape Kargowsky, mouth of the river Kuloï; 7th, the town of Wytegra; 8th, the town of Cholmogory; 9th, the isle of Kolgujev, at the mouth of the river Yussinaja; 10th, the mouth of the river Kambalniza.

Of the ten localities, where the observations were made, eight belong to the polar regions; at four of the places all three magnetic elements were observed; at three the dip and the intensity were observed; and at the remaining three the dip alone could be observed. The violent winds which prevailed prevented the observations being perfected.

1844. Last half-year.—*On the Force of the Currents in a System of Galvanic Piles, juxtaposed and connected together.* By M. E. Lenz. — In this memoir, the author seeks after the analytic expression of the force of currents, when we combine together a certain number, *n*, of galvanic piles, and make the application of that expression to the observation related by M. Crusell, that the intercalation of several persons in a chain diminishes the force of the current in proportion to their number, —an observation which he finds to be confirmed by theory.

ROYAL ACADEMY OF SCIENCES, PARIS. 1845.

Jan. 6.—*On Surface Residence of Magnetic Force.* By Dr. de Haldat. (Vide *Elect. Mag.* vol. i. p. 597.)

Letter by M. Peclet, in reference to *an observation made by M. Pouillet,* in the Preface to the 4th edition of his *Physique;* pp 6.

On Molecular Changes in Wires that have long served as Electrical Conductors. By M. Peltier; p. 1. (Vide *Phil. Mag.* April, 1845, and our next number.)

Jan. 13.—*On the Electro-Plating of Steel.* By M. Desbordeaux. *Note.* (In our next.)

An Aurora Borealis of Dec. 29, 1844, described by M. Coulvier Gravier. *Note.*

Jan. 20.—The Minister of Public Instruction announced the Royal confirmation of *Professor Faraday's nomination as Foreign Associate.* (Vide *Elec. Mag.* vol. i. p. 604.)

An Apparatus for measuring the Velocity of a Projectile. By M. Breguet; p. 5. (In our next.)

On Electromotive Telluric Force. By M. L. Mangrini. (Vide *Elec. Mag.* vol. i. p. 593.)

Jan. 27.—*Reply to M. Peclet's Letter,* read Jan. 6. By M. Pouillet; p. 14.

On the Electro-Plating of Steel. M. Desbordeaux. *Note.* (In our next.)

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Reflections on the Formation of Waterspouts. By M. ARTUR. This communication was referred to a committee.

Feb. 3.—M. ARAGO laid before the Academy *Observations of Tides and Magnetic Declination*, made at Akaroa in 1844, from March 16 to July 18, *by Captain Bérard.*

On the Magnetic Light. By M. COLLA. (Vide *Elec. Mag.* vol. ii. p. 15.)

Feb. 10.—*On the Electro-Plating of Steel.* By M. DESBORDEAUX. (In our next.)

A Note by M. PECLET, *in Rejoinder to that by M. Pouillet*, read Jan. 27.—This note was merely announced, the secretaries believing that this discussion was not interesting to science; for the same reason, we have ourselves simply notified the existence of this controversy. It came within our pale as faithful journalists, inasmuch as the matters in question were mainly electrical.

Feb. 17.—The thanks of Prof. FARADAY were received *on his nomination as Foreign Associate.*

March 3.—*On the Physical Theory of Magneto-Electric and Electro-Magnetic Machines.* By Prof. F. ZANTEDESCHI.—The following is an extract from the memoir:—"The philosophers and mechanicians who, in 1844, in Scotland, Germany, and Italy, constructed new electro-magnetic machines, with the intention of solving the important question of the practical application of electro-magnetism to mechanics, confined themselves to integral and abstract conclusions, that did not include all the experimental data. Researches upon the most advantageous manner of applying the force are in truth very important, as are also researches on the forms of apparatus producing electricity, so that with the least expense the greatest possible amount of effect may be obtained; but these researches are not sufficient for the solution of the problem; we must also know the manner of acting of the force that we wish to apply; and the effects generated by the said force, which are in opposition to the object we have in view. The two former classes of research are the object of study to philosophers in both hemispheres; the third has, in general, been very much neglected; and, on account of this omission, efforts have been redoubled without success. It appears, from what I have heard in my private scientific correspondence, that the commission of German philosophers, appointed by the German diet to examine Wagner's machine,* took into consideration the nature of the electro-magnetic force, and of the effects produced by the apparatus, and were led to conclude that we cannot expect any good results from the application of the electro-magnetic force to mechanics, comparable with those of steam."

March 17.—*On the Production and Nature of Ozone.* By M. DE MARIGNAC.—The results of M. de Marignac's experiments are entirely opposed to Prof. Schoenbein's elementary nature of ozone and compound nature of nitrogen. He finds that the electrolysis

* Vide *Elec. Mag.* vol. i. pp. 172, 303, 609.

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of water gives off ozone, after every possible source of nitrogen has been expelled. The odour from peroxide of lead, boiled in dilute sulphuric acid, appears to him that of nitrous acid, which is produced only so long as air is injected into the liquid; Schoenbein called this ozone. Bi-chromate of potash similarly treated gave no odour. The most convenient means of obtaining ozone is to drive air through a tube containing phosphorus. By this means he obtained the following results:—Dry air does not produce ozone; neither does deoxygenated air; but, when only a feeble quantity of oxygen is present, ozone is abundantly produced. Pure oxygen does not produce it; neither does nitrogen obtained by boiling nitrate of potash with hydrochlorate of ammonia. An artificial mixture of one part oxygen and four nitrogen produces it. The slightest trace of nitrous acid in the air prevents its production. Pure carbonic acid does not produce it; but a mixture of one of oxygen with three or four of carbonic acid produces it. If the carbonic acid is now removed from the mixture, the effect ceases. Hydrogen alone does not produce it; but as soon as the smallest portion of oxygen is introduced into the hydrogen, an abundance of ozone is manifested. Ozonized air, whether moist or dry, loses its odour on passing through a heated tube. Ozone is not altered or absorbed by water, sulphuric acid, chloride of calcium, ammonia, or baryta water. It is absorbed very readily by solution of iodide of potassium, and also by the metals. Ozonized air, when passed through a tube containing silver from the calcination of the acetate, loses its odour; providing always that the air be not dry. In conclusion, the author says that these facts "prove sufficiently that nitrogen is not concerned in these phenomena. It is clear," he adds, "that it is to oxygen alone, or to some particular compound of oxygen and hydrogen, that we must attribute them; but new experiments alone can decide the question."

March 24.—*On two Meteors observed in the vicinity of Layssac; one on the night of Nov.* 19 20, 1844; *the other on Jan.* 16, 1845. By M. Boisse.—The first of these was manifested at two A.M., under a clear sky and eclipsed the light of the full moon then shining. It struck the foliage of an oak, and disappeared with a loud explosion. M. Boisse says, "That he does not, in any way, wish to revive the hypothesis which considered fire-balls and meteorolites themselves as the production of an electric action, but may we not suppose that the appearance of these meteors is sometimes accompanied by a movement of the electrical fluid,—always considering this movement of the fluid not as the efficient cause, but as an accessory circumstance, or even as an effect? May it not happen that foreign bodies, on their suddenly arriving within our atmosphere, determine in it, an electrical reaction, whether by virtue of their own electricity, or by their rapid motion in the air?"

Note concerning the Induced Spark from Magneto-Electric Induction. By M. Santi Linari.—"In the month of November,

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1844, I introduced into the circuit of the magneto-electric current of Clarke's machine, the electro-dynamic cylinder, or spiral with soft iron, beneath which a magnet is placed vertically, and which is wound from bottom to top; wishing, by this means, to obtain its rotation round its axis, and sparks on the mercury by the ends which skim along the surface, as occurs with the galvanic current. Indeed, in consequence of this rotation, continued brilliant sparks appeared on the mercury,—even more brilliant than those produced by Clarke's apparatus itself. I take the liberty of announcing this new fact, which teaches us that magneto-electricity may give a beautiful and brilliant induced, induced spark (*étincelle d'induction d'induction*, Fr.); especially because the experiment that I have just described appears to me more convenient, and having a more prompt result than that which is generally made with the voltaic current. This same induced, induced spark may, in like manner, be obtained on increasing the intensity of the current, produced by the magneto-electro-telluric battery, that M. Palmieri and myself devised."

March 31.—*On Poisoning by Mercury.* By MM. DANGER and FLANDIN.—The electrical portions of this paper will be found in the present number of the *Electrical Magazine*, p. 18.

A note was announced from M. ZANTEDESCHI, *On the Electricity of a Jet of Steam*, and another, with drawings from MM. LINARI and PALMIERI, *On the Phenomena of Telluric Induction.* The contents of these notes are not published in the *Comptes Rendus.*

April 7.—Note by M. ORFILA *Relative to the recent Communication of MM. Danger and Flandin* (vide *Elec. Mag.* vol. ii. p. 20).

April 14.—*Note in reply to the above*, by MM. DANGER and FLANDIN (vide *Elec. Mag.* vol. ii. p. 20).

On the Muscular Current, and on the Proper Current, by M. MATTEUCCI (vide *Elec. Mag.* vol. ii. p. 20).

Experiments on the Electric Discharge of the Jar, by M. MATTEUCCI (vide *Elec. Mag.* vol. ii. p. 22).

On the Fire-ball of October 27th, 1844. By M. PETIT, pp. 12.

April 28.—*On the Vibratory Motions produced in bodies by Electric Currents.* By M. DE LA RIVE (vide *Elec. Mag.* vol. ii. p. 28).

May 5.—M. JOBART, of Lomballe, requests that two memoirs, which he had submitted to the judgment of the Academy, and upon which no reports had yet been made, may be admitted to contend for the prize of Experimental Physiology. One of these memoirs related to the *Anatomy and Physiology of the Electric Organ of the Torpedo.* (The request was referred to the Committee of Experimental Physiology.)

May 12.—M. ARAGO gave a verbal notice to the Academy of the *Experiments recently made on the Electric Telegraph*

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established on the Paris and Rouen railway. They were completely successful. The fidelity with which despatches were transmitted from Paris to Rouen, and from Rouen to Paris, can no longer leave any doubt as to the success of this means of communication. The transmission of the signals was made without any intermediate station, and in a time which does not exceed that which would have been consumed in writing the despatches sent. They are at the present moment occupied in introducing the signals of ordinary telegraphy into this mode of communication; so that the individuals engaged in the present telegraphic lines will not have any new instructions to receive to enable them to transmit despatches with the electric telegraph. M. ARAGO is reported to have estimated the rate of transmission at thirty-two thousand leagues per hour.

On the Employment of the Earth as a Conductor for the Electric Telegraph.—Letter from M. CH. MATTEUCCI to M. Arago (vide *Elec. Mag.* vol. ii. p. 36).

On the Change of Poles, produced by Torsion in an Iron Wire suitably arranged—Note from M. CHORON. "Hitherto," says the writer, "shock and torsion have only been pointed at as being capable of developing a certain coercitative force, in virtue of which soft iron may be magnetized; but I have long observed, that we have merely to bend an iron wire in order to change its magnetic poles. For this effect, I solder an iron wire of commerce by one of its extremities to a plate of any metal, zinc, copper, platinum, &c; and, on presenting this soldered extremity to a horizontal magnetic needle, I observe that the attractions may be changed into repulsions when I bend the wire. I have, moreover, observed a very singular fact, of which no philosopher has yet spoken, and which I resolve to make known only after numerous fruitless attempts to discover the explanation of it. This new fact consists in, that the same soldered extremity may be sometimes attractive, and at other times repulsive, according as it is presented above or below the magnetic needle. This fact is not subject to any kind of regularity; the soldering is not necessary, but it appears to me proper for favouring the production of the phenomenon."

May 26.—*On the Applications of Electricity to the Study of the Phenomena of Terrestrial Decomposition and Recomposition.* By M. BECQUEREL, pp. 27. (An abstract will be given in our next.)

Note on Electro-Chemistry. By M. POUILLET, pp. 6.—In this note, experiments are detailed which lead to the conclusion, "that, in compound bodies, the neutralization of the contrary electric fluid is incomplete." We may give the paper in our next.

Note on the Electro-Chronoscope. By Prof. WHEATSTONE. This note is in reply to the article by M. Breguet, read at the sitting of January 20th. We will give both in our next.

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MM. GILLET and SAINTARD submitted to the judgment of the Academy, *A New System of Signals for the Electric Telegraph.* Each signal, in this system, would consist of a certain number of blows, struck by an apparatus set in action by the transmission of the electric current. Since the interval of time between two successive blows might vary, as between two musical notes, each number, according as it represented what authors term a semibreve, a minim, a crotchet, a quaver, &c., would correspond to so many different signs. We might, consequently, transcribe a despatch just as a musical phrase is written, and very rapidly, by previously ruling lines, the number of which should be equal to the greatest number of blows that could be struck by a single sign.—This notice was referred to the Committee on Electric Telegraphs.

From a verbal communication made to him by M. DE GIRARD, and from two letters addressed to him, the one by M. DE TESSAN, and the other by M. GUILLEMIN, M. ARAGO put forth *A New Explanation of the part played by the Earth in respect to the Circulation of Currents in the Electro-Telegraphic Apparatus*, that are now being tried. A discussion arose on this subject between several members, which will not be dwelt upon just now, as the question is about to be revived very speedily.

PHILOMATIC SOCIETY OF PARIS.

April 19, 1845.—*On a Modified Grove's Pair.* Extract of a letter to M. Peltier, from M. LOUIJET.—" The Grove's pair that I send you presents, in my opinion, important modifications. Principally by the researches of MM. Marianini and Bigeon, we are well acquainted with the influence exercised over the production of an electric current by the relative extent of the metal surfaces immersed in a liquid, and forming of themselves the electro-motive pair. We know that, in proportion as the distance of the plates in the liquid is greater, in order to obtain the maximum effect, we must increase the surface from which the current sets out, in respect to the surface which receives it; and the more so as the liquid is a worse conductor. Thus, in a copper and zinc pair, the copper element ought generally to have the greater extent. Now, in Grove's pairs, such as they have hitherto been constructed, this arrangement is far from being observed; for the negative element is a narrow band of platinum, whilst the positive element, being a hollow cylinder of amalgamated zinc, presents a surface ten or twelve times more considerable than that of the preceding element. I had already remarked to M. Lippens, a young mechanician of Brussels, the great disadvantages, in respect to the energy of the current, presented by this arrangement; and I had assumed, with just reason, that piles of great power might be obtained by changing the arrangement of Grove's pair,—namely, by making the cylinder of platinum, and the central plate of amalgamated zinc. This arrangement has been partly realized by M. Lippens;

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he has, moreover, added certain improvements which are his own. The following is a description of this pair :—

It consists of a cylindrical earthen vessel, holding about a pint. The border of this vessel is furnished with a copper ring, which may be taken off, and which is carefully varnished, and is consequently not to be attacked by the acid vapours: this ring is retained at the top of the vessel by its upper edges, which are turned over; at three points of its exterior circumference are soldered three little plates of argentane, forming a spring, which are tangents to the ring; and the free extremities of which rest strongly on the latter, in consequence of a certain curvature that has been given to them. The part that presses is furnished, interiorly, with a little piece of platinum foil, which is soldered to it, and the whole spring, with the exception of the platinum part, is varnished. By means of these springs, little bands of platinum are fixed on, the width of which may vary, and which, by being bent over, may enter into the vessel, and reach the bottom. A cylindrical porous cell is then placed within the earthen vessel, occupying the greater part of it, so that only a small quantity of nitric acid is required in order to fill it. Within the porous cell is placed a solid cylinder of amalgamated zinc, traversed at its upper part by a varnished metal rod, the ends of which rest on the edges of the tube. To the zinc cylinder is soldered a band of copper, entirely varnished except the upper part, and carrying in a little notch, a double binding screw, which may be taken away, and to which is fixed either the rheophore, when a single element is employed, or the plate corresponding to the platinum in the contiguous element, when several are united in a pile. At one point of the circumference of the copper rim, which embraces the vessel, is also soldered an analogous plate, in like manner carrying a double binding screw at its upper part.

The advantages which this pair presents over the Grove's pair hitherto employed are the following:—1st, for *an equal cost* a much more *energetic* current is given; 2nd, the current may be graduated, by taking away, or lifting out, one or two platinum plates; and we may also have an assortment of platinum plates with different surfaces, so as to make a pair produce its maximum effect; 3rd, the nitric acid is not nearly so much heated as in the ordinary Grove's pair, and consequently produces little or no escape of nitrous vapours; 4th, the zinc cylinder, being solid, is equally dissolved in all its parts, whilst, in the ordinary pair, in which this cylinder is hollow, the interior part is principally eroded by the acid liquid. Moreover, it is very easy of one's self to make the solid cylinders of the modified pair; as the screws are replaced by inoxidizable springs, the pile may act without deterioration for a long space of time; then, when it is finished with, the exterior circle and the platinum plates may be taken away, and the latter washed in plenty of water; the nitric acid likewise which has been employed in the experiment may be pre-

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served in the same vessels, by simply covering them with a plate of glass, or, which is better, their edges may be ground, and covered with a plate of polished glass. This apparatus gives a current of such energy, that with a single pair, having four plates of platinum, I succeeded in producing the incandescence of coke in the open air. In conclusion, I may add, that the price of the pair above described is ten francs; this is the price of an ordinary Grove's pair; but the modified pair has a power of *about three times* that of the latter."

ROYAL ACADEMY OF SCIENCES AT BRUSSELS.

Oct. 1844.—*On the Passivity of Iron*, by M. MARTENS, in reply to a paper by M. Beetz, in *Poggendorff's Annalen.*—M. Beetz had recognised, as had also M. Ryke and M. Martens, that iron might become passive by heat, in a current of dry hydrogen, and there even acquire that bluish tint which had hitherto always been attributed to oxydation; but M. Beetz thinks, contrary to the opinion put forth by M. Martens, that these changes of tint must be attributed to a thin pellicle of oxygen, because he has recognised that this tint is not produced in hydrogen, except at a red heat, and is lost in an atmosphere of hydrogen at a less elevated temperature; which M. Beetz explains by admitting that the oxide of iron, formed at a high temperature, at the expense of slight traces of water, which the hydrogen may have carried with it, is decomposed by this gas at a lower temperature. This explanation does not seem admissible to M. Martens, because hydrogen reduces the oxide of iron, not only at a dull red, but also at a bright red heat; and, moreover, by admitting this as exact, it would remain to be explained how iron wire, that had been allowed *to cool in a current of dry hydrogen*, before putting it in contact with the air, does not present itself with its primitive colour, and in its ordinary active state.

M. Beetz, in order to prove that the dull or bluish tint which heat communicates to iron, can only be the result of oxidation, observes, that this tint is never produced when iron is plunged into boiling oil or mercury, or even into incandescent zinc; but it is easy to conceive that, in boiling oil, the iron cannot become blue, because the temperature is not sufficiently elevated for the molecular or physical state of the metal to be so modified as that this tint shall be produced. Neither does this phenomenon present itself when the iron is placed in a metal bath, either because it enters into alloy with that metal, or because the contact of the latter perhaps opposes the modifying action of the heat.

"If, according to my opinion," says M. Martens, "the bluish tint of iron, raised to a red heat, is not always a result of oxidation of the metal, in like manner I do not regard it as necessary to constitute iron in a state of passivity; for iron endowed with this dull or bluish colour loses it in nitric acid, still, however, remain-

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ing in the passive state; and we may also frequently succeed in removing it, by rubbing it slightly for a very short time with sand paper, without its having entirely lost its state of passivity in respect to nitric acid, of 38° (sp. gr. 1·36)."

Nov. 2, 1844.—*Magnetic Observations*, by M. Houzeau.—These observations were made in the coal-mine at Flénu, and led M. Houzeau to conclude, that for 100 metres (328 ft.) of depth there is an increase of 0·00363 in horizontal intensity, and that it requires a descent of only 27·6 metres (90·5 ft.), to obtain an increase of 0·001 in this intensity. M. Kupffer had found a diminution of 0·001 for 300 metres (984 ft) in altitude; and Mr. Forbes, a diminution in the Pyrenees of 0·001 for 1000 metres. (3280 ft.)

ROYAL ACADEMY OF SCIENCES AT BERLIN.

Oct. 1844.—*On the Development of Heat in an Arc of a Conducting Wire, or of the Ramified Connecting Circuit of an Electric Battery.* By M. Riess.—The author, by aid of anterior experiments, undertaken with the simple wire, which serves to establish the circuit in a battery, in reference to the heat developed in a continuous point of the arc, by the discharge of this battery, was led to the expression—

$$W = \frac{a\,V}{1 + b\,V} \cdot \frac{q^2}{s}; \quad . \quad . \quad (I.)$$

in which q expresses the quantity of electricity accumulated on the surface s, a and b constants to be determined empirically, and in which V and V depend on the nature of the variable part of the circuit whose degree of heat is in question. Each of these two latter quantities, which are termed the retarding values of the wire in question, is equal to the product of the length of the wire by a constant appropriated to its nature, divided by the square of its semi-diameter. This retarding value is not always given immediately, except for a simple wire; and consequently the formula cannot be applied when the wire that establishes the circuit is composed of several wires, arranged one beside the other, and under the form of ramifications running parallel to each other. To generalize the formula, therefore, it is necessary that the following problem be resolved: — The retarding values of V' V'', &c. of any number of wires being given, it is required, what is the heat developed in a point of the circuit, when these wires are simultaneously substituted as ramifications or divisions of this same wire. All the other questions upon the heat in the other portions, in the trunk and the branches, are resolved by the employment of formula I.

It follows from this, that an hypothesis corresponding with that which led to the preceding formula, leads also to the expression of heating in question; and that all that remains to us is to de-

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monstrate its legitimacy by appropriate experiments. The denominator $1 + b\,V$ of the first formula has been considered as the time during which the whole quantity of the electricity q has been discharged through the constant and the variable portions of the circuit. In order that the formula may be applicable to a wire presenting ramifications, it is simply sufficient, in place of $b\,V$, to introduce the time z, during which the branches, in a common discharge, contain the whole quantity of the electricity. The first branch, which, in the time $b\,V$, discharged the quantity q, in the time z, can receive only the portion $\frac{q\,z}{b\,V}$; in like manner, the second branch can receive only the portion $\frac{q\,z}{b\,V'}$, and so on; so that, in order that the total quantity q of the electricity may be exhausted, we must have $\frac{z}{b}\left[\frac{1}{V}\right] = 1$; or $z = \frac{b}{\left[\frac{1}{V}\right]}$ in which $\left[\frac{1}{V}\right]$ designates the sum of all the $\frac{1}{V}$ present.

The branch n^e consequently receives the quantity of electricity $q_n = \frac{q}{V_n\left[\frac{1}{V}\right]}$. The substitution of z instead of $b\,V$ in the first formula gives the quantity of heat in the trunk of a branched wire:

$$W = \frac{a\,V}{1+\frac{b}{\left[\frac{1}{V}\right]}} \cdot \frac{q^2}{s} \quad . \quad . \quad \text{(II.)}$$

The accuracy of this formula is found demonstrated by all the experiments, that the author has undertaken on this subject.

If we pass from one point of the trunk-wire to another, the quantity of heat developed by a discharge of this wire changes only in relation to the charge brought about in V, whilst each point is traversed by the same quantity of electricity. But it is no longer the same when we pass to another point of a branch, whilst each branch contains only a portion of the quantity of electricity accumulated in the battery. The n^e branch, (whose retarding value is V_n) receives only the quantity q_n of electricity, the value of which when introduced instead of q, into the second formula, gives, as an expression of the quantity of heat W_n, in a point of the n^e branch,

$$W_n = \frac{a\,V\,\frac{1}{Vn^2}}{\left[\frac{1}{V}\right]\left(\left[\frac{1}{V}\right]+b\right)} \cdot \frac{q^2}{s} \quad . \quad \text{(III.)}$$

This formula was likewise put to proof in an empirical manner,

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and confirmed. The second and third formulæ (for the first may be considered as a special case) embrace all the calorific researches hitherto instituted on the circuit which establishes communication between the poles of an electric battery. With whatever simplicity these formulæ may have been deduced from the expression previously verified, it was nevertheless necessary carefully to reproduce the circumstances of the experiments. The division of the current of the discharge is subject to disturbances, which, when the experiments are not conducted with the necessary precautions, are sufficiently sensible to overthrow all the simple laws which are manifested in the formulæ. We had learned by previous experiments, that when another secondary wire is introduced into a portion of a circuit, there circulated in the ring thus formed a secondary current in the same direction as the principal current, and of a nature such as to modify it in different manners. Each branch of the circuit may be considered as a secondary circuit of another branch; and, when there are only two branches, we have yet to consider in each branch three currents, two of which are in the same direction, and the third runs in a contrary direction. It is only when the two branches have equal length that two opposite currents are equal to each other and are neutralised, so that the action of the principal current appears in all its distinctness; whilst, in all other cases, the difference between the first two currents acts as a powerful disturbing cause. This disturbance, according to the preceding experiments of the author, follows the force of the secondary current, and is of so complicated a nature that it is not possible in general to express it even qualitatively. In experiments of this kind, therefore, we must endeavour to render this disturbance as small as possible; now, as all circumstances being equal, the quantity of electricity which circulates in a secondary current is proportional to the virtual length of the derived portion of the total circuit, it is proper to operate with short branches, which, in order that they may present retarding values, that are but little sensible, must be very fine. Independently of their serving as branches, the fine wires always require that they be not exposed to too high a charge. It is by paying attention to all these circumstances that all the experiments related in the memoir have been conducted,—experiments which have confirmed, in the most evident manner, the simple laws that are manifested by the ramification of the current of the discharge.

In conclusion, the author believes that he ought to put us on our guard against an extension which has several times been attempted to be given to the first formula, or to that which has been attempted to be applied to the expressions given in this memoir. All the formulæ are established on empirical bases, and consequently ought not to be extended to cases which had not been included when they were established. As, in general, the quantities of heat which become free by the electric discharge in continuous wires have alone been considered, it follows that the formulæ will

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not teach anything as to the development of heat in discontinuous pieces of wire (connected together by solder or by pressure) so long as they are not supported by other experiments than those of the author.

Oct. 1844.—*On the Charging Current of the Electric Battery.*—By M. Dove.—If the conductor of an electric machine is connected, by means of a platinum wire, with the inner coating of an uncharged battery of Leyden jars, the free electricity that is developed on one of the extremities of the wire (the one near the conductor) combines with that of the other extremity (the one near its inner coating). On the other hand, if we connect the outer with the inner coating of a charged battery, the positive electricity, which becomes free at one extremity of the wire, neutralizes that, which is in like manner free and negative at the other extremity. In both cases, the wire is traversed by an electric current. An uncharged battery is found to be charged by means of the first current, and a charged battery is discharged by means of the second, so that the former may be termed the *charging current* or the *charger*, and the latter, the *discharging current* or the *discharger*. If, in comparison with a great number of experiments on the discharging current, we as yet possess but very few on the charging current; this is simply because the current elicited in this manner, or by charging a battery by cascade, is extremely feeble. In order to reply to the question whether such a charging current possesses properties identical with those of the discharging current, it is indispensable that we should restrict the duration of this charging current into a space of time, as short as that for the discharging current. We may accomplish this by the following plan, by means of which, too, we obtain currents of any intensity we may desire, with all the characters of an instantaneous duration, without producing sparks in an uninterrupted metallic conductor.

Of two batteries, equal to each other, and resting upon distinct insulators, and each composed of sixteen litre (quart) jars, one was charged to a given degree, by means of sparks produced from a Lane's jar. The outer coating of the charged battery, being insulated afresh, was then put into connection, by means of a wire, with the external coating of the uncharged battery; and the communication of the interior coatings was then brought about by a metal discharging rod. At the moment when the spark passes to the internal communicating wire, and the electricity accumulated upon one internal coating is diffused over the two internal coatings, the same thing is produced, but without the apparance of sparks, by means of the external communicating wire, on the two outer coatings. This process not only succeeds in respect to the principal discharges that occur at a striking distance, but also for the small consecutive sparks that present themselves or follow the first sparks, until the metallic contact is completely brought about. Charging currents are obtained in both communicating wires, and in which the same quantities of electricity pass; but with this difference that the production of sparks in the exterior currents has been avoided.

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With regard to the intensity of these currents, it depends on the original charge, and on the relation between the size of the coatings in the charged and in the uncharged battery. This intensity is greater for the same charge of the first battery, in proportion as the coated surface of the second battery is greater. After the passage of the two charging currents, both batteries are charged. If they are now separated, the positive and negative electricities of the wire, by which the circuit is established, are neutralized by the discharges of the second battery; electricities which at the first moved separately in the two communicating wires. In order to be assured of the identity of action between the charging and the discharging currents, we have, consequently, merely to compare the effects resulting from the discharge of the second battery, with the effect of each of the two currents formerly mentioned.

In each of the following experiments, all the rheometric apparatus were separately insulated.

1. *Induction.*—A cylindrical inducing spiral was introduced into the exterior communicating wire; this spiral was exteriorly surrounded by a secondary spiral. The induced discharge was found to become stronger, by aid of a bundle of iron wires, and to be weakened with a solid rod of iron. Such, in fact, are the properties which principally distinguish the induced current, produced by the discharging current of the electric battery from induced currents produced by other electrical forces. They are equally applicable to the charging current; moreover, they are independent of the circumstance of knowing whether the current was produced by a spark or not. To demonstrate this, an inducing spiral, similar to the preceding, was simultaneously introduced into the interior communicating wire, which spiral was enveloped in a secondary spiral, also identical with that above mentioned. These two secondary spirals, being mutually identical, were placed in communication in the same manner. Each of the two charging currents produced by induction a secondary current within the secondary spiral; and as these two currents circulated in a contrary direction, the two secondary currents were united in the secondary wire, as in a metallic whole. A perfect equilibrium of current was the result; whence it may be concluded that the interruption of the arc of communication, by a point emitting sparks, does not exercise any influence over the induced effects of the charging current.

2. *Sparks.*—If the exterior communicating wire is interrupted, a snapping spark of a white lustre is manifested at the point where this wire is interrupted, at the moment when the spark passes from the interior communicating wire. If a moist vegetable thread is introduced into the interior communicating wire, the spark then acquires a yellow-reddish colour, and makes but a feeble noise. The same change is presented at the point of interruption of the exterior communicating wire, into which no piece of moist thread has been introduced.

3. *Galvanic Effects.*—If a galvanometer is introduced within

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the point of interruption of the exterior communicating wire, this instrument is in no way affected when the interior coatings are placed in communication with the production of a white snapping spark, but without the interposition of a moist vegetable thread; but, on the contrary, it is strongly affected when a moist vegetable thread is introduced into the interior wire.

4. *Magnetization of Steel.*—The magnetization of a steel needle, contained in a spiral, is brought about very energetically in the former, and very feebly in the latter case.

5. *Physiological Effects.*—If the interruption of the exterior wire is completed by means of the human body, by aid of handles, there is a very sharp shock in the former, and a very insignificant one in the latter case.

It was remarked that there was no difference in the energy of the shock whether the handles were grasped with the hand or merely between the fingers; only, in the latter case, the sensation in the fingers was extremely painful.

6. *Piercing of Imperfect Conductors.*—If a battery is discharged through a card by means of two metal knobs brought near to it, but whose line of junction passes obliquely through this card, it follows, as Lullin has demonstrated, that the piercing of this card does not take place at an equal distance between the two knobs, but occurs in vacuo, near to the knob of the negative coating. The same thing occurs with the charging current. If the coating of the charged battery is positive, the card is pierced near the knob of the uncharged battery; whilst, if it is negative, it is near the knob of the charged battery.

Trémery has shown that, when the discharge of the battery is made through tin-foil, placed between two sheets of paper, and in a direction inclined with respect to the sheet, the sheet is as often pierced in front of the negative as in front of the positive knob; and he explains this effect by supposing that the two free electricities, whilst they are acting in order to separate that of the tin-foil, are not only attracted by each other, but are still more attracted by the electricities produced by this separation. The same thing takes place in respect to the charging current. Two points may be constantly observed on the tin-foil, when the foil is distorted, bent, or torn,—one corresponding with the knob of the charged, and the other with that of the uncharged battery.

In respect to the unipolarity of soap, which is obtained by galvanic effects, I examined, after having introduced this body into the exterior communicating wire, if any difference was manifested, whether the charging current were positive or negative. The result was an identical current in both cases,—the only difference of which was that it was determined in a contrary direction.

7. *Heat.*—The heat of the charging current in the exterior communicating wire, when measured by aid of an electrical thermometer, was the same as when this communicating wire was used for the discharge of the second battery. In the former case the

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discharge occurred without, and in the latter with the formation of sparks.

The following are the results of the experiments that have been made:—1st. That phenomena of resistance may be manifested in a homogeneous communicating wire, absolutely similar to those which are observed in a wire composed of several conductors.

2nd. That primary currents of momentary duration are developed in any intensity without formation of sparks, and present properties identical with those of currents, which are propagated only with the formation of sparks.

3rd. If a free positive electricity is excited at the extremity *a* of a wire *a b*, and the wire is united at the extremity *b*, the same effects of current follow as if free positive electricity had originated at the extremity *a*, and free negative electricity at the extremity *b*, and both had been neutralized in the wire—that is to say, that the charging current, both positive and negative, presents effects identical with those of the discharging current.

If the exterior coatings of two insulated batteries of the same power are put into communication, of which the interior of one is charged to the same degree positively as that of the other is negatively, when the internal coatings are made to communicate by means of a discharging-rod, we obtain a discharging current in the exterior communicating wire without formation of sparks, whilst in the interior wire a current is produced in the opposite direction with the formation of sparks. The two batteries are then perfectly discharged. We can, therefore, execute on these two discharging currents the same experiment as on the two charging currents previously mentioned. But it is very difficult, perhaps even impossible, to arrive at perfect similitude in the condition of the two batteries. When this cannot be managed, it happens that, after the appearance of the spark of the inner coatings, the two batteries are charged in the same manner, and according to the more powerful of the two; for to the discharging current there almost instantly succeeds a charging current, determined in the same direction, whilst the excess of one battery that remains, in consequence of the discharge, is immediately divided between the coatings of the two batteries.

The charging current presents also this advantage, that we may obtain it, so to speak, gratuitously, whilst the two batteries, when, after the appearance of the spark of the inner coating, they remain united, are charged exactly in the same manner as if the inner coating of the associated battery had been in immediate communication with the prime conductor during the rotation of the machine. If this charging current is applied, we obtain for a given number of turns of the disc, the same effect as formerly with the double current.

GEOLOGICAL SOCIETY OF FRANCE.

May 19, 1845.—M. J. Delanoue offered a few remarks upon the following passage from a letter by M. Becquérel, recently

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inserted in the Bulletin of the Society :*—" As an effect of transport, the electric origin of which cannot be doubted, there is that which takes place when the auriferous pyrites of Berezoff have lost their sulphur, in order to take oxygen and water, without changing their form. We there see the action of an electric current, which brings one element and carries away another. The contact of the gold and the pyrites, in presence of water, at the surface of the mineral, is sufficient to commence the action, which is then propagated gradually to the middle." M. Delanone says :—" I do not comprehend the possibility of the aggregation of an ore, the first element of which forms a pile, which must not attract but decompose every other similar element tending to group with it. And, with regard to the propagation in itself, how are we to attribute it to the presence of gold, or of any other foreign body establishing an electric current, when we so frequently see perfectly pure crystals of pyrites converted in like manner, without alteration of form, into hydrate of sesquioxide of iron ? "

ASSOCIATION OF AMERICAN GEOLOGISTS AND NATURALISTS.

Fifth Session, at Washington, 1844.—*On the Connections existing between Geology and Magnetism.* By Mr. Locke.—The author's magnetic observations embrace the country between Cambridge, Mass., and the extreme west point of Jowa ; and between the middle of Kentucky on the north, and the north bank of Lake Superior. By keeping account of the geology of the place of each observation, he has traced out relations between the magnetic indications and the composition of the subjacent soil. As, for instance, on the whole horizontal calcareous region, which is generally free of trapp or other igneous rocks for several hundred miles, and of all local dislocation, the magnetic curves regularly ascend toward the north. On the contrary, where the subjacent rocks are of igneous formation, a magnetic curve, from Baltimore to New York, presents ascending and descending undulations, resembling the contours of the primitive surrounding mountains ; and one along the south bank of Lake Superior, presents not only undulations, but also a sort of inflections of so abrupt a character as to present veritable nodes or knees. The subjacent rocks are principally trapp, and they bear traces of igneous origin. In studying the trapp rocks near New York, he found that, when they are good conductors of magnetism, and especially when they are elevated and of an oblong form, they become feebly magnetic by the induction of the earth itself, and acquire a contrary polarity to that of the earth, thus diminishing in a sensible degree the effect of the terrestrial magnetism of the neighbourhood. He found the maximum of magnetic intensity to be near Lake Superior. Thus there would exist three important poles or points almost on the same meridian and very nearly also at an equal distance apart, and

* Second Series, vol. ii. p. 222.

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directly to the north of the United States, between the longitudes 85° and 90°; first, the true north or astronomical pole; second, Ross's pole of perpendicular dip or magnetic convergence, 20° south of the astronomical pole; third, the pole of maximum magnetic intensity, 21½° south of Ross's pole, 41½° south of the north pole, or at 47½° of north latitude.

TRIBUNAL CORRECTIONEL DE LA SEINE (c. 8.) JUNE 6. 1845.

M. Perrot, President.

MM. Christofle & Co. *v.* M. Bertrand.

THE plaintiffs have a license to use MM. Elkington and Ruolz's patents for electro-gilding and plating; and have already maintained several actions. The present action occupied the tribunal during four sittings; and is interesting both in respect to the questions which it raises, and to the developments which it has given of the respective pretensions of individuals.

The following are the facts:—

Several seizures were made, at the request of M. Christofle, in the workshops of one Bertrand, a gilder; and a jury of three, MM. Chevalier, Barrel, and Henry, appointed by the magistrate who has this affair in hand, have drawn out a detailed report of the objects seized, and placed it on the rolls of the court. Their opinion is favourable to the patentees.

M. Emanuel Arago opened the case for the plaintiffs by detailing the affair, and commenting on the report of the jury.

M. Marie, on the part of the defendant, put forth three propositions, and gave evidence in support of them:—

1st. *There was no invention of galvanic gilding, either on the part of Mr. Elkington or of M. de Ruolz; and consequently their patents cannot be regarded as valuable.* In support of which, he cited the chemical labours of Brugnatelli,* and De la Rive,† and a publication by M. Louyet, of Brussels. Brugnatelli and De la Rive had discovered the principle of the new gilding; and this principle, in his opinion, contains the germ of all the ulterior labours of MM. Elkington and De Ruolz, which ought to be considered as simple applications, of a more or less happy kind, but not patentable.

2nd. *Supposing that there was an invention, and that the patents were valid, M. Bertrand cannot be condemned for infringement; for, before the patents were taken out, he gilt by the same processes that are now in use; he ought to enjoy the benefit of the anterior possession.* In support of this, M. Marie presented to the Tribunal numerous attestations in proof that, before the patents were taken out, M. Bertrand had gilt objects that could not be

* Vide *Biblioteca di Campagna*, &c., vol. x. p. 185—6. Milan, 1807.

† Vide *Annales de Chim. et Phys.*, vol. lxxiii. p. 398. 1840.

‡ Vide *Bulletins de l'Acad. de Bruxelles*, Dec. 4, 1841, vol. viii. p. 448.

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gilt by the old process, such as filigrees and solderings of tin; he also put forth invoices proving that, before the patents, M. Bertrand had purchased certain chemical products, and especially the cyanides, the useful application of which to galvanic gilding had afterwards been pointed out by MM. Elkington and De Ruolz. And, according to the learned counsel, M. Bertrand is not the only one who had employed these substances anterior to the patent. M. Perrot, of Rouen, has just published a pamphlet, in which he maintains having gilt with the cyanides before the patents were out.*

3rd. *In the next place, the report of the jury cannot serve as the ground for an immediate solution; and this for several reasons,—first, because it says nothing in a precise manner on the question of novelty, and then, because it is* ex parte; *if it is* ex parte—*if M. Bertrand would not assist the operations of the jury; it is because, in his opinion, the questions were not well put by the magistrate. A second report is therefore necessary; and a fresh jury ought to be named.* He here gave a critical examination of the report, which appeared to him incomplete, and by no means conclusive.

M. Emanuel Arago, in reply, said that he could not comprehend how the discovery of MM. Elkington and De Ruolz could be denied, to whom the Academy of Sciences had awarded two grand Monthyon prizes, upon the report of M. Dumas, as one of a commission, consisting of MM. Thenard, Darcet, Chevreul, Pelouze,

* We give the following extracts from M. Perrot's pamphlet, entitled "*Machine Lithographique, Machine à Imprimer les Etoffes et Dorure Galvanique,*" published by Lange, Levy and Co., Paris, 1844.

"1805. Brugnatelli.—The first idea of gilding by the galvanic pile is traced to Brugnatelli. He states his having gilt silver medals with a pile of several elements.†

"1840.—M. De la Rive.—This learned philosopher gilds silver, copper, and brass with chloride of gold, by means of a galvanic apparatus of a single element. The piece to be gilt forms the negative pole of the single element.

"1840, *August.* M. Perrot.—I obtain on gold, silver, copper, and on very oxidizable metals, such as iron and steel, very fine gildings, without the interposition of any metal. Not any of these gildings could hitherto be obtained, except by the employment of the battery and the alkaline cyanides.

"1840, *September.* Mr. Elkington.—He demands a further patent (*brevet d'addition*, Fr.), in which he appropriates M. De la Rive's process. But, instead of dissolving the gold in chlorine, he uses prussiate of potash (alkaline cyanides); and he replaces the bladder used by M. De la Rive by porous earth, as had been long the practice in electrotype.

"1840, *December* 15*th.* M. De Ruolz.—He proposes to cover silver objects with a film of copper, so as to render them fit to be gilt by M. De la Rive's process."

† "To one part of the saturated solution of gold in nitro-muriatic acid, add six parts of solution of ammonia, by which the solution is decomposed, and oxide of gold is precipitated, and a portion is set free, forming ammoniuret of gold."—Brugnatelli's gilding solution, extracted by *Editor of Elec. Mag.*, from Zantedeschi's Electricity, vol. ii. p. 468.

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and Pelletier. In order that the defence be admissible, we must admit that all these philosophers were ignorant of the labours of De la Rive. Now, this same commission, that decreed a prize of 8000 fr. to Mr. Elkington, and another of 8000 fr. to M. De Ruolz, had given M. De la Rive a prize of 4000 fr., in recompense for the discovery of a scientific principle—a scientific principle inapplicable, as they explain in their report, to the actual art of gilding; whilst, in the processes of MM. Elkington and Ruolz, they find all the conditions of a good applicable gilding—a marketable gilding by electricity,—a gilding that was impossible before their processes.

M. E. Arago, in support of his assertion, cited several passages from the report of the Academy of Sciences, proving that the liquids described by MM. Elkington and De Ruolz, cover objects to be gilt with a continuous and perfectly adhering layer of gold, without in any way attacking and corroding the objects; whilst M. De la Rive's liquor reacts on the objects, attacks them, and cannot cover them with that adhesive and continuous layer of gold which is alone acceptable in commerce. He further added, that if De la Rive and Brugnatelli did produce good gildings, let M. Bertrand adopt their processes, and the patentees would not in any way interfere with their operations. With respect to the anterior possessions, the learned counsel canvassed the specimens produced by M. Marie, and which do not appear to him in any way decisive. With regard to M. Perrot, he appears in exactly the same position as M. Bertrand; but, even supposing he were right, this would have no bearing on M. Bertrand; for if M. Perrot had shown his gildings before the patents, he had never told, had never published how he had obtained them. M. Arago concluded by repelling the demand for a fresh jury.

After a fresh reply from M. Marie, the King's Counsel, Camusat Busseroles gave his opinion in favour of M. Christofle:—

"The Tribunal, &c., as concerns Cloménil,—inasmuch as there is no proof that he acted otherwise than as workman to Bertrand, executing the orders of his patron, dismiss him without expenses. As concerns Bertrand,—and first, with respect to the demand for a new jury to report also on the process claimed by Bertrand; inasmuch as Bertrand's process, so far as it constitutes a personal and distinct discovery, is not in question; inasmuch, on the other hand, as there does not appear any legitimate motive for recommencing the investigation that was made, as it is regular, and contains sufficient elements, when joined to the other specimens and documents of the process, to throw light upon the decision that is to be given; and mainly, inasmuch as it follows from the debates, the specimens and the documents produced, that the preparations seized at Bertrand's are conformable to those, for which the prosecutors or their authorities have obtained some of their patents; that Bertrand maintains that these patented preparations have not the character of true inventions; that he also opposes the exception of forfeiture, and in all cases anterior possession.

"But, in respect to the question of invention,—inasmuch as, in the matter of discoveries, not only are the principles susceptible of being patented; that their consequences, their divers applications and combinations, may also constitute new ideas and found privative rights; that, if it is true that the cyanides of gold were for a long time in science, as well as was the galvanic pile, the efficacious application of these cyanides to gilding and plating metals, in a manner, more-

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over, such as to endow society with a new and so valuable a branch of industry, assuredly merits the title of invention. In respect to the exception of nullity or forfeiture,—inasmuch as the rule is, that a patent is worth nothing, if the discovery, invention, or application is not new; that is to say, if, anteriorly to the date of the deposit of the demand, it has received in France or abroad a sufficient publicity to be executed; but, in fact, inasmuch as it is true that the chemists Brugnatelli and De la Rive had opened the career, their labours, which were curious and interesting in a scientific point of view, were as yet, to speak truly, but simple trials; that it follows from all the documents of the process, that before Ruolz and Elkington, gilding and plating, by aid of the galvanic pile, had remained in such a state of imperfection that the arts and commerce could not gain any thing from them, and that it was the cyanides of these latter, their preparations and combinations, which raised the discovery from its sterility, in order to extend it by a new discovery into the industrial arts; whence it follows that there is in very truth a novelty in their particular invention. As concerning anterior possession,—inasmuch as, rightfully, this means is purely personal; that we cannot invoke the possession of another; inasmuch as, therefore, Bertrand cannot create a title to the possession of MM. Perrot of Rouen, and Louyet of Brussels, if such exists; and moreover, as nothing proves that these two philosophers were in possession of the means of MM. Ruolz and Elkington before their demand for the delivery of patents; further, that nothing establishes their having spread them abroad, either by printed writings or otherwise, so as to attain the degree of publicity necessary to their being henceforth considered as new; finally, inasmuch as, with respect to Bertrand, he by no means justifies his personal possession; that the productions which he makes with this intent are utterly insufficient. That it follows from all which precedes, that there is truly a patentable invention in the processes of Ruolz and Elkington; a new invention without anterior possession on the part of a third individual, to-wit, Bertrand; that thus he has rendered himself guilty of the crime of infringement, provided against and punished by the articles 40 and 49 of the Law of July 5, 1844:

"For these reasons, without delaying or having regard to the demaud for a new investigation, nor to the exception of the defence, which are rejected as being badly founded,

"Condemns Bertrand to a fine of 300 fr., pronounces confiscation of the preparations and objects seized; orders that they be handed over to Christofle and Co;

"And, inasmuch as it has been established, that the latter have suffered prejudice from the deed of the aforesaid Bertrand, that reparation is due to them, and that the Tribunal is prepared to appreciate the value;

"Condemns the aforesaid Bertrand to pay to Christofle and Co. the sum of 3000 fr. as costs and damages; and

"Condemns him to the expenses;

"Fixes to one year the duration of the writ of arrest."—From *Le Droit*, June 7, 1845.

NEW BOOKS.

Recherches sur les Grêles auxquels sont exposés les Etats de Terre Ferme de S. M. le roi de Sardaigne, by M. le Chevalier Despines.—The Electro-Physiology of Man, by J. D. Humphreys. Pickering, Piccadilly. 1845. 7*s.*—*Histoire du Magnetisme dont le phénomènes sont rendus sensibles par le mouvement. M. de Haldat. Nancy,* 1845. 8*vo.*—*Trattato del Magnetismo e della Elettricita. Fr. Zantedeschi. Part II.* 8*vo. pp.* 346. *Plates. Venice,* 1845.

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NOTICES OF EXPERIMENTS, &c.

The late Professor Daniell.—"The late John Frederick Daniell, professor of chemistry, in King's College, London, lecturer on chemistry and geology, at the Hon. East India Company's Seminary at Addiscombe, one of the examiners in the University of London, foreign secretary of the Royal Society, D.C.L. (Oxon), was born in Essex Street, Strand, March 12, 1790. At an early age he became a pupil of Professor Brande, in whose society he made several tours, and of whom he spoke, as one endeared to him by kindred pursuits and early recollections, the day before his death. In 1816, associated with this gentleman, he started "The Journal of the Royal Institution," the first twenty volumes of which were published under their joint superintendence. He married in the following year (Sept. 4), Charlotte, youngest daughter of Sir W. Role, surveyor of the navy; and subsequently became managing director of the Continental Gas Company, to forward the interests of which he visited the principal cities of France and Germany, with Sir W. Congreve and Colonel Landmann, making those arrangements by which many of them have since been lighted. On the formation of King's College, in 1831, he was appointed professor of chemistry, and found himself at length in the position he was so suited to occupy. His inaugural lecture, eminently characteristic of the Christian philosopher, gave a good earnest of the spirit in which his instructions would be conveyed.

"Of the extent of Professor Daniell's scientific labours, some idea may be formed from the fact that, independent of his "Meteorological Essays," and "Introduction to the Study of Chemical Philosophy," he communicated to various scientific periodicals upwards of forty original papers; of these thirteen relate to meteorological subjects, nine to electricity, and the remainder to chemistry and other branches of physical science. Of their intrinsic importance some notion may be obtained from the circumstance that he received all three of the medals in the gift of the Royal Society. In 1820 he published an account of his new hygrometer, an instrument which, for the first time, rendered regular and accurate observations on the dryness and moisture of the air practicable. It has since been extensively employed in all climates; and has enabled hygrometry to take an exact and definitive form. It still remains the only accurate instrument for making such observations. In 1823 appeared the first edition of his "Meteorological Essays," of which he was engaged in revising proofs of the third edition at the time of his death.* This work was the first synthetic attempt to account for meteorological phenomena as a whole, the known laws of which regulate the constitution of gases

* This edition was so far advanced at the time of his death that it will soon be in the hands of the public.—Ed. *Elec. Mag.*—It is now ready, July 1.

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and vapours. In the following year (1824) appeared his essay on Artificial Climate, in the "Horticultural Transactions;" the practical bearing of which on culture in general is daily becoming better appreciated; and which, according to Dr. Lindley, has done more for the improvement of this art than any single circumstance besides. He received the Society's silver medal for this paper. In 1830 and 1831 he published his new pyrometer—an instrument still the best for measuring high temperatures, such as those of fusing metals and furnaces in general. The Royal Society deemed this an invention of such utility and importance that they, in 1832, conferred on him the Romford medal for the most important discovery relating to heat that had been made throughout the civilized world during the three preceding years. In 1836 appeared a paper in the "Philosophical Transactions," describing his valuable improvement in the voltaic battery, by which he showed the means of obtaining a constant and unlimited supply of electricity. The importance of this discovery was recognised immediately throughout the whole scientific world. In appreciation of its merit the Royal Society, in 1837, honoured him with the Copley medal, for the most important scientific discovery of any description made in any part of the world during the previous year. Several other valuable papers appeared in the "Philosophical Transactions" for the following years; and for two of these he, in 1842, received one of the Royal medals."—*Times.*

Electricity.—To this science we are indebted for some very valuable discoveries in the purification of metals during fusion. I will repeat the experiment by again passing the current of electricity. You perceive how instantaneously the pure and the impure particles are separated. This is accounted for by what I have just explained as to positive and negative attraction. But it is not only for the purification of metals that electricity is a powerful and mysterious agent in the hands of science; it extends to their atomic combinations also. This was discovered about eight years since by Mons. Watson, à Londres; and to it the world is indebted for his simulated silver, called "Albata" (argent de Londres, désigné par le nom de Albata). No patent was taken out for this process, as the specification would have declared the method to competitors; but it has been kept a profound secret, and a large fortune is being realized by the inventor, through the manufacture of every article formerly made only of silver. I consider it possible that the efforts of the alchymists of old will at last be perfected through the aid of electricity. It was not in the proportions that they failed, but in the means of purification, and the perfect combination of the atoms.—*Mons. de Bonneville's Lectures, Académie des Arts à Paris.*

Mariner's Compass.—Mr. Dent has patented a mode of suspending the needle and card "in a similar way to the balance of a chronometer, and with equal delicacy, both ends of the pivot acting on diamonds, and the holes jewelled, by which means the card is

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constrained to move very nearly in the horizontal plane, since in this respect it is entirely under the control of the gymbals." And "to remove the error arising from the non-agreement of the *marked* or *maker's*, with the *magnetic axis*, a simple contrivance is effected for the *inversion of the card*, so that either side of it may be placed *above* or *below*. Since the marked axis of the needle is in each of these positions at equal distances from, but on opposite sides of the magnetic meridian, a mean of an equal number of observations in both positions of the card will evidently eliminate any error of this nature."

Hydro-Electrical Machine.—An extraordinary sized electrical machine has just been completed for the United States, by Messrs. Watson and Lambert, of this town, which is called after the great American philosopher, "Benjamin Franklin." The effects of this steam electrical machine are, indeed, truly surprising, far exceeding any thing ever yet accomplished by man in this important branch of science. It was put into operation on Thursday, in the open yard of the manufactory. The weather was most unpropitious—it never ceased raining; but, independent of this difficulty, the spark obtained from the prime conductor was twenty inches in length, at the rate of two hundred in a minute! It was shipped, yesterday, on board the *Spring*, for New York, where, we understand, it is to be used in the Polytechnic Institution, which is being established in that city. The Americans will, in the acquisition of this wonderful instrument, possess by far the most powerful electrical machine in the world, it being more than three times the capacity of the one used at the Royal Polytechnic Institution in London, which has been the source of gratified astonishment to all who have beheld its truly magnificent results. As many of our readers may not have had an opportunity of inspecting this Leviathan machine, we may briefly describe its construction. The steam-boiler, which is insulated by glass pillars, is constructed upon the locomotive principle, having a fire contained within, and passing through tubes, which are surrounded by water, into the chimney. There is a glass gauge attached for regulating the proper quantity of water in the boiler; also a safety valve to relieve the pressure of the steam above a certain point. There are two hollow metal arms issuing from the upper part of the boiler, to each of which are attached seventy small iron tubes, the ends of which are furnished with proper jets, and by the passage of the steam through these the electricity is produced. There is a junction pipe, fitted with a cock, for the purpose of shutting off one of the arms, and using only one half of the power of the machine, or by keeping it open, the whole power. The rows of pipes which are fixed to the metal arms, and through which the steam issues, are enclosed and surrounded by a casing or box, for the purpose of holding water for producing a condensation of the steam in its passage through the pipes, previous to its emission from the caps on their extremities. This condenser, as it is called, is supplied with water as it is required, by vessels placed above

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having pipes communicating with them; and there are pipes connected with the condensers for collecting and carrying away into the chimney the vapour produced by the water becoming heated in consequence of the passage of the steam through them. The steam from the jets is blown against four rows of forks placed on framework, and supported by insulated legs. When the machine is in action the positive electricity is rapidly produced, and may be collected at the ball in connection with the forks, and the negative electricity may be obtained from all parts of the boiler.—*From the Newcastle Journal of May* 24, 1845.

Chess by Telegraph.—On Wednesday and Thursday, April 9 and 10, 1845, chess was played between Gosport and Nine-Elms, through the intervention of the Electro-magnetic Telegraph, by Mr. Walker and Mr. Staunton, and other well known players.

Electro-magnetic Telegraph.— Anecdote. — On April 19th, a passenger by the Great Western Railway, from Paddington, discovered that he had lost his purse, containing notes and cash to the amount of nearly 1000*l.* He alighted, in great agitation, at Slough; and, on telegraphing his distress to Paddington, was agreeably surprised to find, that he had left his purse on the counter at the terminus, and that it was safe in the hands of the clerk.

Electro-culture.—In various parts of the kingdom experiments are being made upon the application of electricity in its several forms to horticulture and agriculture. Mr. Forster's experiment is being repeated in several places, amongst others, by a scientific farmer near Brighton; it is said that the electrified crop has already a strong and vigorous appearance. But we shall hear more of this in due time. We have ourselves some experiments in hand, and have our eye on several others; and hope, in the autumn, to have collected a few facts on this all important point.—*Editor Electrical Mag.*

Molecular changes in metals that have acted as conductors.—M. Peltier describes the brittleness that wire acquires after having carried for a long time a voltaic current, as in the experiment of the lead tree. He also found that wires for collecting atmosphere electricity become very brittle after long exposure to the atmospheric and electrical current, and he was obliged to renew them in two years; brass wires broke in six months. Conductors screened from the weather kept their ductibility longer; he intends employing, for his atmospheric arrangements, tinned wires, by which the cementation may be avoided, which acts as well as the electricity in destroying them. — *Comptes Rendus*, Jan. 6, 1845.

Muscular current from living human muscle.—The following extract, from a letter addressed to us by Professor Matteucci, dated Pisa, June 3rd, is an admirable illustration of true devotion to science:—"An accident has lately happened to me, which, without being of a very serious nature, has put me into the situation of

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forming on myself the muscular current. I have received a kick from a horse in the muscles of my leg; and, with the galvanoscopic frog, I have obtained very definite signs of the muscular current."

Capillary attraction on electrical phenomena.—Dr. Draper illustrates this opinion, by referring to several cases in which bodies in cohesion evince opposite electric states when separated,—as, two plates of glass,—glass and mercury,—melted sulphur and glass,—ice and glass, &c.: by showing that cohesion may be brought about; by giving bodies opposite electrical characters,—as mercury will be wetted by water, when the two are connected with the respective ends of a voltaic pile: and by the explanation this theory gives to several obscure facts,—as the great height at which mercury will stand in a narrow barometer-tube, &c.—*Vide Phil. Mag.*, March 1845.

Galvanic Alloy of Lead and Iron.—"To a solution of lead in nitric acid is added a solution of protosulphate of iron, so that the resulting solution, when cold, shall not be very concentrated. If it is too strong, the sulphate of lead is not precipitated, but becomes black, and does not answer the purpose. The liquid that remains after the precipitation, in the form of white powder of the sulphate of lead, contains all the iron and lead necessary for galvanoplastic application. The alloy obtained in this manner is much harder than lead, does not melt at a much higher heat than that at which the latter metal melts; and it is magnetic. A cutting instrument does not touch it. The most curious thing is that these two metals, the most difficult to manage for forming alloys, yield most readily under the influence of the voltaic current."—*Annali de Fisica*, No. iv., 1844.

Contact experiment.—As "a direct experiment to demonstrate that the simple contact of different metals is sufficient for charging a Leyden jar," M. Marianini took five Leyden jars coated with zinc outside, and with silver inside: he placed them, one within the other, and made communication between the inner coating of the innermost, and the outer of the outermost, by means of a silver wire; and the apparatus charged itself. His test of charge was the contraction of a frog placed in the circuit.—*Il Cimento*, Ann. iii. 1845, p. 67.

Memory of Galvani.—Count Giovanni Galvani suggested to the Italian Society, September 26, 1844, that a subscription should be entered into to raise a monument in Bologna to Galvani. The President reminded the Society that the Bolognese have already honoured their illustrious fellow-citizen with medals and other monumental tributes.—*Il Cimento*, Ann. iii. 1845, p. 67.

Omnia.—A regular mass of platinum, about eight inches square and one inch thick, and weighing 501 ounces, was upon the library table of the Royal Institution on Friday, April 11th; and was upon Professor Faraday's lecture table, on the following day. He stated it to be worth about 800*l.*—The Wollaston medal of the

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Geological Society is henceforth to be of palladium, in honour of Wollaston's discovery of that metal. — M. Pétit has given the parallax of several new fire-balls, in the *Comptes Rendus* for the 21st of last October.—The clock-tower of Laon has been destroyed by lightning: the vane of the Tower of Graces was struck, and the cross knocked off on May 17, 1844.—During the earthquake at Grenada, West Indies, Jan. 19, 1844, the needles of the compasses of the *Thames* steamer, which was then among the islands, revolved on their centres with great rapidity. *St. George's Chronicle.*—Berzelius has lately determined the atomic weight of iron to be 28·025; and Erdmann, that of zinc to be 32·52. *Proceed. Stockholm Acad.*—*Electro-thism*, or relative electromotive power, a term that has been applied to metals in respect to their relative value in forming pairs, is derived from ἤλεκτρον, *amber*, and ὠθισμὸς, propulsio, *propelling power.*—The thermo-electric pile has again been made available by Melloni, in some recent researches on radiant heat. *Comptes Rendus*, 3rd March, 1845.—The words "Loadstone for the Gout," are duly framed and glazed in the shop-window of an optician in Coventry Street. —The papers say that Mr. Bain's electric telegraph is about to be established on the Edinburgh and Glasgow Railway,—His Holiness forbids the introduction into his dominions of the dangerous and heretical process of gilding by galvanism!—Dr. Hare has published, in Silliman's Journal, remarks on Faraday's speculations on electric conduction, and the nature of matter.*—M. Matteucci has communicated to the Royal Society two memoirs on the muscular current, one of which was read on the 5th and the other on the 19th of June.—In the *Phil. Mag.* of April is a note on Static Induction by M. Matteucci; in our next we will give a translation from "Il Cimento" of a paper of his, containing further experiments on this subject.—Mr. Gassiot gives a "Soirée Electrique," on the 28th of June, to Professors Dove, Kupffer, Schœnbein, and other foreign philosophers, who had attended the meeting of the British Association.—At the Royal Society, on June 19th, was a paper by Mr. Grove, "On the Gas Voltaic Battery; and on the Voltaic action of Phosphorus, Sulphur, and the Hydrocarbons."—We communicated to the British Association, a note on secondary action, as illustrated by the Voltaic Reduction of Brass; it was read June 21st.—We witnessed the action of Mr. Beningfield's Electric Gun, on June 14th, and found it to be a most formidable instrument; it will give "short balls," or single balls or volleys, that will kill at a mile at pleasure; when not wanted, it can be put out of action immediately; and it is stated to be ready for service again at a moment's notice.—Prof. Morse has discovered a mode of conveying the action of the electric telegraph across a river by means of the water only.—Prof. Page has been able so to arrange the coils of the magneto-electric machine, as to produce a current that will give to an electro-magnet a lifting power of 1000 lbs.

* Vide *Elec. Mag.* vol. i. p. 295.

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ELECTRICAL MAGAZINE.

October, 1845.

CXI. *Notice of an Apparatus intended for Measuring the Velocity of a Projectile in different points of its path.* By M. L. Breguet. From "*Les Comptes Rendus*," Jan. 20, 1845. Vol. xx. No. 3, p. 157.

(Translation.)

The memoir* that M. Pouillet has lately read before the Academy, on the employment of electricity as a means of determining extremely short periods of time, impresses upon me the necessity of publishing this note, relating to an instrument the object of which was the same, and which I constructed, about a year ago, for the Russian government, conjointly with M. Konstantinoff, a very distinguished artillery officer.

M. Konstantinoff had arrived from England. When he came to see me, he already had the idea of an instrument suited for measuring the initial velocity of projectiles, as well as their velocity in different points of their path; having become acquainted with Mr. Wheatstone, who is so well known for his ingenious works, they had several conversations together on this subject; and he saw, at the house of that philosopher, an apparatus† which, by means of electric currents interrupted and re-established, permitted the measurement of the time of the inflammation of gunpowder, with a great degree of exactness. M. Konstantinoff thought, however, that, by another arrangement, he might obtain still greater exactness, and measure much shorter intervals. He consulted me on this project; and, believing the solution of the problem that he proposed to me to be possible, we commenced working together in the month of March, 1843.

* Vide *Elec. Mag.* Vol. I. p. 565. † Vide *Elec. Mag.* Vol. I. p. 473.

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The problem was this:—to arrange an instrument which might indicate and preserve thirty or forty successive observations, made in very close spaces of time, of a phenomenon happening at a greater or less distance from the place where the instrument of observation is situated. It naturally occurred to us to employ electricity for this purpose.

It was furthermore necessary to unite to this physical part the mechanical part, which might become very complicated, but which, however, was not so much so as the solution in question induced us to presume.

Private reasons had prevented my making this machine known; but, as at present nothing remains to prevent its publicity, I will endeavour to convey as exact an idea of it as I possibly can without the aid of figures; moreover, it has been seen by MM. Arago, Regnault, and Morin, whose authority may be quoted if necessary to establish what I advance. M. Regnault especially has, so to speak, followed it in all its phases, and has assisted at almost all the experimental essays that had in view the verification of the principles on which it was established.

We thought of employing an apparatus with a revolving plate, similar to that of M. Morin; and as we required several successive indications distant from each other, we had thought of making the style alter its position, by approaching or withdrawing from the centre at each new mark; but this means did not appear to us sufficient, in that the marks made at the centre and those made at the circumference were not within the same limits of error; for the smaller the radius was of the arc traced by the style, the greater would be the chance of error.

We then placed the indications in identical positions, by constructing a cylinder of sufficient length to make forty or fifty distinct observations, all of which should appertain to the same radius. In June, 1843, we commenced the construction of our machine, which was not completed until May, 1844.

The following is its arrangement:—

The apparatus is mounted on a cast-iron frame, and is composed of six distinct parts:—

1st. Of a system of toothed wheels, set in motion by a cord rolled round a cylinder, and to which is suspended the moving weight.

2nd. Of a cylinder, 1 metre (3·28 ft.) in circumference, and 0·36 metre (1·18 ft.) in length, divided on its surface into a thousand parts, which are therefore millimetres. In order to diminish its friction on the pivots, it is supported by a system

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of jewels. On its axis is a pinion, which communicates with the wheel-work beneath ; at one extremity is a flier with four wings ; and at the other a plate of the same diameter as the cylinder.

3rd. Of a small metallic road, parallel to the axis of the cylinder ; the two lines of which this road is formed are insulated from each other by ivory.

4th. Of a small carriage mounted on three copper wheels and rolling on the two lines ; it carries three electro-magnets, and two styles independent of each other, but each dependent on one of the electro-magnets. The third electro-magnet is placed beneath the carriage, and serves to retain it until the moment when it is required to start.

5th. Of an anchor-escapement, the arm of which of soft iron, oscillating between two electro-magnets, is attracted now to the right and now to the left, according as a current passes round the right hand or the left hand magnet. This alternate motion allows one tooth of the wheel to escape each time, upon the axis of which wheel is a small roller, on which is wound a silk thread affixed to the carriage, that is drawn along by a weight. The passage of the current from one magnet to the other is made, at each half turn of the cylinder, by means of a commutator placed on its axis ; in this way the carriage advances a constant quantity at each half turn, and the rate of its progress is proportional to that of the cylinder.

6th. Finally, of a particular arrangement, in order to ensure uniform motion, independently of any chronometric apparatus, and which affords the means of determining the limits of error in the final results.

Thus the chronometric apparatus has for its basis the principle employed by M. Morin for establishing his revolving plates, with this difference, that the plate is replaced by a cylinder, and that the flier carries wings, which are portions of spirals, whose tangent is inclined 45° to the radius vector. This we have found preferable for obtaining the uniform motion more rapidly ; because by this means the air presents a greater resistance than with plane wings. The position of the cord carrying the motive weight is rendered constant by passing over a pulley, which slides on a strong rod. In proportion as it is developed, the cord is tackled on two conical pulleys, and may be of two or of six strands. Finally, the apparatus, although made on a large scale, is constructed with all the care that we devote to our most delicate works.

The fusee, which in machines of this kind is generally in gear with the wheels of the cylinder, in the present case

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carries a very simple means of being out of gear; so that, when once the weight is wound up, the fusee has no communication with the machine; and thus all needless friction is obviated.

We have constructed several small pieces of mechanism similar to each other, but separated one from the other, and each contained in a small numbered box; they served to establish the circuit for one sieve, when that in advance of it had been pierced. These boxes contained an ivory wheel with rachet teeth, and having one metallic tooth; upon its axis was an iron pallet, having a catch entering into the teeth of the wheel. Another catch, independent of the former, was a retaining catch, and it re-established a voltaic circuit, as soon as the metal tooth touched it.

In front of the pallet was an electro-magnet which attracted it when the current circulated around it, and allowed it to go back when a wire was cut in a sieve. During this movement the ivory wheel advanced and brought the metal tooth near to the retaining catch.

A series of distances, beginning from the charge, being determined, a conductor will pass before the bullet, and another before the cannon's mouth; and for the other points sieves will be placed, the surface of which will augment with the distance.

The sieves are large squares, over the surface of which the wire that conducts the electricity passes in all directions, so as to present the appearance of a net, the meshes of which are smaller than the diameter of the projectile, in order to make sure of the wire being cut in whatever place the sieve may be pierced. The current that circulates in one sieve, passing at the same time round the electro-magnet of one of the styles, maintains the latter by magnetization, at a distance from the cylinder; whence it is evident that at the moment when the sieve is pierced, the current being interrupted, the style will fall and make a mark on the cylinder. The projectile, continuing its route, will pierce another sieve, which by communicating with the second style, will make it fall on the cylinder, where it will also make a mark; and it is by means of the distance between these two marks and of the known velocity of the cylinder, that the velocity of the projectile in its passage from one sieve to the next in order is calculated.

We might have one current and one style for each sieve; but it were more simple to employ only two currents, whatever might be the number of sieves; and for this purpose the little boxes mentioned above, are employed in the following manner: —

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Each box is placed between two sieves, beginning from the second; and by this means, as soon as the second sieve is pierced, the current is established for the third, and the first style is again raised; the third sieve being pierced, the second style is raised, the first falls, and the current traverses the fourth sieve. This operation is thus repeated unto the last.

The two styles having each their proper current, and being consequently independent of each other, infinitely small spaces may be measured, which could not possibly be accomplished with a single style and a current that would be interrupted and then established again.

We have seen that the cylinder is divided into a thousand parts, its circumference being one metre. Each millimetre represents $\frac{1}{1000}$ of a second when it makes one rotation per second, $\frac{1}{2000}$ when it makes two, $\frac{1}{3000}$ when it makes three, &c.

Against its circumference, and against that of the plate, which, as we are aware, is insulated, certain springs are made to rub; on each of these circumferences is an ivory arc, so as to produce an interruption in the electric currents which pass by the electro-magnets of the styles. This arrangement is intended for verifying the uniformity of motion and the interval of time which the styles occupy in falling on the cylinder; a quantity which it is essential to know exactly, or at least it is necessary that we should know the limits of error within which it oscillates, in order to make the necessary corrections when we are measuring the number of divisions between two neighbouring marks of the styles, which are to give the velocity in the space traversed by the projectile.

We, therefore, see that at each turn, or each time the piece of ivory arrives under the spring, the current is interrupted, the style falls, and is raised again at the end of the insulating arc, in order to fall again at the following turn.

Now, if we observe carefully the division of the cylinder on which the style falls while the cylinder is at rest, and then the point on which it falls while the cylinder is in motion, its velocity of rotation in a second of time being known, we shall easily obtain the measurement of the time that the style has consumed in falling during the arc above measured. Thus it is that, when the cylinder makes two and a half turns per second, the arc measured is 30 mm.; hence $\frac{30}{2500} = 0^s.012$ as the time which the style occupied in falling on the cylinder. These proofs were repeated a thousand times.

In order to observe whether the movement is uniform, the cylinder is put into rotation, and when it is supposed to be very equal the circuits are established. The following are the results of this examination:—

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The carriage conveying the electro-magnets and the styles is put in motion, and at each turn the styles make their marks on the cylinder, but in different places, and in a horizontal direction.

When it arrives at the end of the cylinder, and the indications are examined, we ought to find all the marks in the same right line, if the motion is uniform; under the form of an helicoidal line, if it is accelerated or retarded; or sinuous, if it is unequal. By this means we obtain a true chronometric apparatus, which verifies itself.

We have observed the motion on velocities of two and a half and three turns per second, and on making the style fall we found all the marks in the same right line; sometimes there were differences of 1 mm., which indicated a variation in the motion of $\frac{1}{2500} = 0^s.0004$ at that instant.

In order to appreciate the moment when the velocity becomes uniform, we observed the rotations of the axis immediately in front of the cylinder with a register; but in order to escape this somewhat tedious operation, I conceived the idea of placing a commutator on the axis, and arranging a registrar (the needle of which makes dots on a square) with a system of electro-magnets.

At each turn of the axis the commutator formed an electric circuit, which, circulating around the electro-magnets, produced a powerful attraction, and the extreme end of the lever pressed on the button of the registrar; the dots thus made on the square were marked with great regularity.

This latter instrument may, as I imagine, be employed with advantage in manufactories; for, by means of conductors, leading from the office of the superintendent, and communicating either with the fly-wheel or the cylinder of a steam-engine, he may know at every moment in the day, and without disturbing himself, the velocity of either one or the other. For the sake of greater convenience, the pile might be replaced by electro-magnetic currents.

This instrument might also serve usefully in observations that might be made on the velocity of hydraulic wheels, according to the nature of the operations that are executed by the tools which they drive.—[A.]

CXII. *Note on the Electro-Magnetic Chronoscope.* By Prof. WHEATSTONE. From "*Les Comptes Rendus,*" May 26, 1845. Vol. xx., No. 21, p. 1554.

(TRANSLATION.)

I PERCEIVE, in the *Comptes Rendus de l'Académie des Sciences,* that, at the sitting of Jan. 20, a communication was read

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from M. Breguet,* in which he attributes to Captain Konstantinoff and to himself the invention of the electro-magnetic chronoscope, an instrument which I had myself invented and completed several years previously, for the purpose of measuring rapid motions, and especially the velocity of projectiles.

It was in the commencement of 1840 that I invented this instrument. My chronoscope was then composed of a clock-movement, acting on an index-needle, which moved on or stopped, according as an electro-magnet acted on a piece of soft iron, attracting it when a current traversed the helix of the magnet, and abandoning it to itself when the current ceased, as in my electro-magnetic telegraph, of which this invention may be considered as one of the derivations. The duration of the current was thus measured by the extent of the circle traversed by the hand of the chronoscope.

A relation was established between the duration of the current, and that of the movement of the projectile by the following means: a wooden ring embraced the mouth of a loaded gun, and a stretched wire connected the two opposite sides of this ring, thus passing in front of the mouth of the gun. At a proper distance, the target was erected, and so arranged that the least motion given to it would establish a permanent contact between a little metal spring and another piece of metal. One of the extremities of the wire of the electro-magnet was attached to one of the poles of a small voltaic battery; to the other extremity of the electro-magnet were attached two wires, one of which communicated with the little spring of the target, and the other with one of the ends of the wire stretched before the mouth of the gun; from the other extremity of the voltaic battery two wires were also given off, one of which came to the piece of metal affixed to the target, and the other to the opposite extremity of the wire that passed in front of the gun. Thus, antecedently to the explosion of the gun, there was established between the gun and the target an uninterrupted circuit of wire, and of which the wire in front of the gun formed part. When once the target was struck by the bullet, the second circuit was complete; but during the passage of the projectile through the air, and during this time alone, both circuits were interrupted; and the duration of this interruption was indicated by the chronoscope.

I had already demonstrated, by my electro-magnetic telegraph, that when the magnets are properly arranged, they may be brought into action by a very feeble battery, even though the wires should describe a circuit of several miles.

* Vide preceding article.—*Ed. Elec. Mag.*

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Consequently, the gun, the target, and the chronoscope, may be placed at any required distances from each other. On account of the very great rapidity with which electricity is propagated, as was proved by my experiments, published in the *Philosophical Transactions* of 1834, no sensible error can result from its successive transmission.

During a visit which I paid to Brussels in the month of September, 1840, I described this apparatus to my friend M. Quetelet, who made it known on the 7th of October, to the Academy of Sciences of that town,—a communication mentioned in the bulletin of that sitting.*

In a visit which I afterwards paid to Paris (May 1841), I explained this apparatus, and showed the drawings to several members of the Academy of Sciences of Paris, who came to see me at the *Collège de France;* where, thanks to the kindness of M. Regnault, I had an opportunity of repeating before them several of my electro-magnetic experiments. Among the persons present was M. Pouillet, who asked of me permission to copy my drawings, to which I willingly consented. I learned from him, in December last, that these drawings were still in his possession.

On my return to England, my friend Captain Chapman,† of the Royal Artillery, convinced of the utility of this instrument, was very desirous that it should be introduced in the practice of the Artillery at Woolwich; and he took much pains to accomplish this. We had an interview on this subject with the late Lord Vivian, then Master General of the Ordnance; and on July 17, 1841, I explained at the Institution of the Royal Artillery, the construction of the instrument and its different applications. Twenty-two officers assisted at this meeting; in the report of it (a report, of which I possess a copy) it is said that my chronoscope "indicated $\frac{1}{7300}$ of a second," and that my object was to "show its application to the practical uses of artillery," namely, to determine the time employed by a projectile in passing over the different sections of its course, as well as its initial velocity. At the same meeting, I showed a "chronoscope intended to measure the velocity of lights, such as those produced by the ignition of powder." This instru-

* Vide *Elec. Mag.* Vol. I. p. 611.

† I had for a long time kept up a correspondence with Captain Chapman on this subject. In one of his letters of August 27, 1840, after having communicated to me his views on the manner of conducting his experiments, he says:—"We shall thus obtain the velocity of a projectile in each of the sections of its course; and I am bold to believe that we shall arrive at a knowledge of the effect of gravitation on the projectile, much more satisfactory than all that has been hitherto obtained."

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ment, the only one that M. Breguet attributes to me,* had, however, nothing in common with electric currents, as he supposes; it was simply a series of wheels, carrying on their axes three light paper discs, of about an inch in diameter. The times of their respective revolutions were as 1, 10, and 100; the disc, whose movement was the most rapid, made 200 revolutions per second; on each disc was traced a radius: when they were illuminated by an electric spark all the radii appeared at rest, on account of the excessively small duration of that kind of light (as is explained in my memoir, *On the Velocity of Electricity and on the duration of the Electric Light*, published in the *Philosophical Transactions* of 1834); but when they were illuminated by a light of a duration of the two-hundredth part of a second, the third disc appeared uniformly tinted, while the second disc showed a shaded section of 36°. When the light lasted only the two-thousandth part of a second, a similar section appeared on the third disc.

For several reasons, the experiments with my electro-magnetic chronoscope were not pursued at Woolwich. In 1842, I became acquainted with M. de Konstantinoff, captain of Artillery in the Imperial Guard of his Majesty the Emperor of Russia, and attaché to the staff of General de Winspaer; he took much interest in this affair, expressed a great desire to have a complete apparatus, in order to undertake himself, on his return to Russia, a series of experiments such as those which I had in view. As I had not myself the time to follow out these experiments, and as no one in England, more skilful and better circumstanced for this, showed the desire of following them out, I willingly yielded to his demand, in the hope that some important results to science might be obtained. The only condition that I attached to my consent was that M. de Konstantinoff should not publish any description of the instrument until I myself had done so. The instrument that I furnished to M. Konstantinoff, and which was directed to him at Paris, in January, 1843, was differently constructed from that previously described, although essentially the same in principle.

I had found, by experience, that when a piece of soft iron had been attracted by an electro-magnet, and the current was then made to cease, although the iron appeared to fall immediately, its contact was maintained for a time which was frequently equal to a considerable fraction of a second. The duration of this adherence increased with the energy of the voltaic current, and with the weakness of the reacting spring.

* Vide ante, p. 81.

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To reduce it to a minimum, it was necessary to employ a very feeble current, and to augment the resistance of the circuit until the attractive force of the magnet was reduced so as to exceed the reacting force of the spring by a very feeble quantity; but then the magnet had no longer sufficient force to attract the iron, when the projectile struck the target. However, I surmounted this difficulty in the following manner:—I arranged the wires of the circuit in such a manner that, before the bullet left the gun, the current of a single element of very small dimensions, and reduced to a suitable degree by means of a rheostat,* also interposed in the circuit, acted on the electro-magnet; but when the ball arrrived at the target, six elements, without the resistance of the rheostat, acted simultaneously upon the magnet. But even with these precautions, which are efficacious to a certain degree, there is still some time lost during the attraction of the iron by the magnet, as well as during its adherence after the current had ceased. The difference of these two errors would render approximations such as the $\frac{1}{500}$ or $\frac{1}{1000}$ of a second altogether uncertain. However, the error arising from this source may be easily reduced to at least $\frac{1}{60}$ or $\frac{1}{100}$ of a second; and, in my opinion, a chronoscope which divides the second into sixty parts, and which may be proved never to give rise to an error exceeding a single one of these divisions, is preferable to an instrument presenting more minute divisions, and which would give rise to errors embracing a good number of these divisions. Guided by these experiments, I was in a condition to construct a very simple and very efficacious chronoscope. A very simple escapement was put in motion by a weight suspended to the end of a thread wound, in a hollow helix, round a cylinder fixed on the axis of an escapement wheel. On this axis was also adapted an index, which consequently advanced one division for every escapement. When it was necessary to prolong the time of the experiment, the escapement wheel and the cylinder were established on different axes, and their inter-communication was brought about by means of a wheel and pinion; in this case two hands were employed. By means of this construction we avoid the acceleration of motion that might take place if there were no escapement; and the index passes over each division in the same time. The weight was so arranged as to be able to regulate itself; and the value of a single

* An explanation of this instrument will be found in my *Account of several new Instruments and Processes for determining the Constants of a Voltaic Circuit*, published in the *Philosophical Transactions* of 1843, 2nd part; and translated into the *Annales de Chim. et de Phys.*—Vide *Elec. Mag.*, Vol. I., p. 203.

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division was obtained by dividing the time of the entire fall by the number of divisions passed over in this interval; but methods still more exact may be employed.

By means of this instrument, I have measured the time occupied by a pistol ball in traversing different ranges, with different charges of powder. The repetition of these experiments gave out results that were very constant, rarely presenting a difference of more than one division of the chronoscope.* I also measured the fall of a ball from different heights; and the law of accelerated velocities was obtained with mathematical rigour. With the apparatus that I employed for this latter experiment, I could measure the fall of a ball from the height of an inch. It would be difficult, without the assistance of drawings, to give an idea of the different arrangements that I have adopted to render the instrument applicable to different series of experiments; but I may mention that, among the applications, I propose to employ it for measuring the velocity of sound through air, water, and masses of rock, with an approximation that has never been obtained heretofore.

Independently of the instrument which I furnished to M. de Konstantinoff, in April, 1843, Prof. Christie deposited one in the Museum of Natural Philosophy of the Military Academy at Woolwich; and another was made for Mr. R. Addams, who has since constantly used it in his lectures at the United Service Museum† and elsewhere.

I will mention a modification of the instrument that is important for certain series of experiments:—instead of breaking the continuity of the circuit, and re-establishing it immediately, as we have hitherto said, the electro-magnet is maintained in equilibrio by means of two equal and opposite currents; on interrupting the first circuit the equilibrium is destroyed, and on interrupting the second the current occasioned by the interruption of the equilibrium ceases. The second circuit is broken by a ball traversing a frame, on which is stretched a very fine wire arranged in parallel lines very close to each other, and forming part of the circuit. This arrangement furnishes means for employing a chronoscope totally different from the former. Two pendulums, one a half-second pendulum, and the other having a little more accelerated motion, are each maintained at the extremities of their

* These experiments, in which I was assisted by Sir James South and Mr. Purday, the celebrated gunsmith, took place in October, 1842, in the grounds belonging to the observatory at Camden Hill.

† Vide *Elec. Mag.*, vol. i., p. 473, which refers to the instrument seen at the United Service Museum.

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arcs of oscillation, by means of an electro-magnet. When the ball escapes from the gun, one of the pendulums is liberated, and when it breaks the wire of the frame the other pendulum is also liberated. The number of oscillations of one of the pendulums is then counted, until the motion of the two pendulums coincides; and from this fact, the time which separates the commencement of the first oscillations of the two pendulums is easily calculated.

The instruments which I really constructed had no other object than to indicate the time that had elapsed between the initial and final motion of a ball in traversing its path. M. de Konstantinoff desired an instrument measuring the times corresponding to the successive divisions of the path. Although I then thought, and still am of opinion, that it is preferable to determine them by means of successive discharges, yet I did not undertake the construction of one, because of its higher cost and its greater complexity, although it was the object of frequent conversations between us. It was in order to realise these ideas that M. de Konstantinoff, after his departure from England, and during his stay in Paris, subsequently communicated with M. Breguet, in order to profit by the well-known skill and ingenuity of that engineer. I am perfectly persuaded that M. de Konstantinoff never had the intention of attributing this invention to himself, and that it is entirely without his approbation, and without his knowledge, that M. Breguet has just done so.*

With regard to the instrument described by M. Breguet, I consider it as being much less exact, much more complicated, and more costly, than any of those which I have previously invented. When it is reduced to determine merely the initial and final movements of a ball, M. Breguet's instrument is furnished with five electro-magnets, each with its mechanism, whilst mine attains the same result with a single electro-magnet; and when the different divisions of a same path are to be studied, M. Breguet proposes a complementary magnet, and makes other additions to each of the partitions which the ball was to traverse. Had M. Breguet been better informed on the means by which I would obtain a series of successive measurements corresponding to a same path, he would have found that what he proposes to obtain, even with a dozen electro-magnets, would have been obtained in a much more

* I here give an extract from a writing given me by M. de Konstantinoff, before quitting London:—

"M. Wheatstone having had the kindness to make for me a complete apparatus of his invention to measure the fall of bodies, and the initial velocities of projectiles I undertake," &c.

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efficacious manner by means of one alone. The following was my plan:—

A cylinder executes a rotary motion round a screw, so as to advance a quarter of an inch per revolution; to one of the extremities of the cylinder is adapted a toothed wheel of a diameter a little greater than that of the cylinder, and which communicates with a pinion whose length is equal to the total portion of the axis which the cylinder must pass over in its successive revolutions; this pinion communicates with a wheel work, put in motion by a weight suspended at the extremity of a wire winding around a cylinder, and the wheel work is furnished with a regulator which equalizes its motion; a pencil, adapted to the extremity of a small electro-magnet, is brought into contact with the cylinder, and traces upon it a helix, which is interrupted every time that the current ceases. I borrowed the idea of the chronoscopic part of this apparatus from an instrument intended for measuring small intervals of time, invented by the late Dr. Young, and which is described and drawn in his *Course on Natural Philosophy*. It may be easily comprehended, from what I have already stated, in what manner the commencement and the end of the motion of a projectile are indicated by this instrument. The intermediate processes are registered in the following manner. At any given points on the line of the passage of a projectile are erected frames closed by wire net-work; the projectile breaks the wires on passing through the frames; as many voltaic batteries are employed as there are pairs of frames, the wires of which pairs of frames communicate with the poles of these voltaic batteries, and with the wire of the electro-magnet, in such a manner that the electric current traverses the wire helix, or ceases to traverse it, according as the equilibrium is alternately destroyed or established by the successive rupture of the wires of the frames. In order to obtain this result, it is necessary that the resistance of the different wires be suitably proportioned.

In conclusion, I may add that the application of my electro-magnetic telegraph with a view of registering at a distance the number of the revolutions of a machine, or of all other periodic movements, has been executed by me under very various forms for several years. An apparatus with this object in view, registering up to ten thousand, may have been seen in the Museum of Natural Philosophy at King's College, since 1840; and was shown to M. de Konstantinoff during his stay in London.—[A.]

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CXIII. *On Prof.* PAGE'S *Arrangement of the Coil for the Production of a Quantity Current.* From the "Report of the U. S. Commissioner of Patents," for the year 1844, p. 440.—Laid before Congress in 1845. Communicated by W. G. Lettsom, Esq.

Improvement in the Magneto-Electric Machine, and application of this instrument to operate on the Magnetic Telegraph.

THE magneto-electric machine was originally contrived by Mr. Saxton, soon after the commencement of the interesting discovery of Faraday, that magnetism was capable of exciting electricity. The conditions necessary for obtaining electricity in this way, were chiefly the disturbance of magnetic forces in a bar of soft iron, surrounded by coils of wire. A number of mechanical contrivances were resorted to in order to effect this disturbance, by causing the bars of iron thus surrounded to approach to, and recede from, the poles of powerful magnets; but the ingenuity of Mr. Saxton far exceeded them all, by giving to the coils and enclosed bar a rotatory movement about the poles of a U-form magnet. This instrument afforded bright sparks and strong shocks; but the currents of electricity thus obtained, could not be converted to any useful purpose, as in each half-revolution of the coils the currents were in opposite directions. In 1838, Prof. Page published in *Silliman's Journal* an account of an improved form of the machine, doing away with many existing objections, and furthermore rendering it at once a useful instrument, by a contrivance for conducting these opposing currents into one channel or direction, which part of the contrivance was called the unitress. The current produced in this way was capable of performing the work, to a certain extent, of the power developed by the galvanic battery; and the machine was found adequate to the furnishing of shocks for medical purposes, for exhibiting the decomposition of water, furnishing the elements oxygen and hydrogen at their respective poles, and producing definite electro-chemical results. These two last results could not be obtained without the aid of the unitress. But with this improvement, the instrument was still wanting in one property of the galvanic battery, viz:—that property which chemists call quantity, or that power upon which depends its ability to magnetize, and also to heat platinum wires. This last property has been given to the machine by the recent contrivance of Prof. Page. The machine, in its novel construction, under his improvement, developed

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what is called, by way of distinction, the current of intensity, but had a very feeble magnetizing power. By a peculiar contrivance of the coils (not to be made public until his rights are in some way secured), the current of quantity is obtained in its maximum, while, at the same time, the intensity is so much diminished that it gives scarcely any shock, and decomposes feebly. It has been successfully tried with the magnetic telegraph of Prof. Morse, and operates equally well with the battery. It affords, by simply turning a crank attached to the machine, a constant current of galvanic electricity; and, as there is no consumption of material necessary to obtain this power, it will doubtless supersede the use of the galvanic battery, which, in the event of constant employment, would be very expensive, from the waste of zinc, platinum, acids, mercury, and other materials used in its construction. It particularly recommends itself for magnetizing purposes, as it requires no knowledge of chemistry to insure the result, being merely mechanical in its action, and is always ready for action without previous preparation; the turning of a crank being the only requisite when the machine is in order. It is not liable to get out of order; does not diminish perceptibly in power when in constant use, and actually gains power when standing at rest. It will be particularly gratifying to the man of science, as it enables him to have always at hand a constant power for the investigation of its properties without any labour of preparation. We notice among the beautiful results of this machine, that it charges an electro-magnet so as to sustain a weight of 1000 lbs. and it ignites to a white heat large platinum wires, and may be used successfully for blasting at a distance; and should Government ever adopt such a system of defence as to need the galvanic power, it must supersede the battery in that case. Prof. Page demonstrates, by mathematical reasoning, that the new contrivance of the coils affords the very maximum of quantity to be obtained by magnetic excitation.

CXIV. *Morse's Electro-Magnetic Telegraph.*—From "The Report of the United States' Commissioner of Patents," for the year 1844, p. 442. Communicated by W. G. Lettsom, Esq.

THE following figure represents the most simple form of the electro-magnet, with its appropriate machinery for telegraphic pur-

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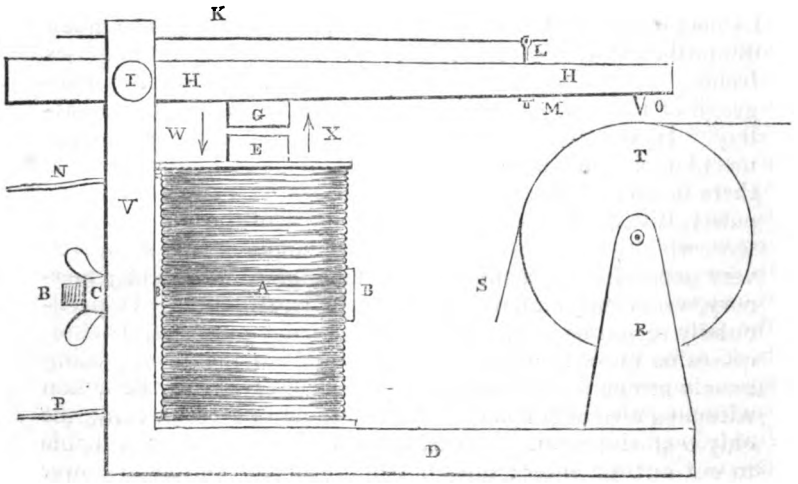
poses. A represents a side-view of the bent iron bar, surrounded with its coils of copper wire, standing upon a platform, D. V being an upright arm secured to D, to which the mag-



net or soft iron is permanently fastened by means of the bolt, B B B, passing between the prongs of the curved iron, and through the board, V, and adjusting-screw, C. E is the projecting prong of the iron after it has passed through the coils,—one only being seen. The other prong is directly behind E. G represents the end of the iron bar or keeper extending back so far as to cover both the projecting ends of the horse-shoe-formed magnet. This iron bar or keeper is fastened to the lever, H H, which is delicately adjusted so as to rise and fall by a pivot at I. K represents a steel spring over the lever, H H, and passes through a loophole, L, formed from a brass wire; the lower part of the brass wire being secured to the lever, H H, by means of a screw at M. O is a hardened steel point, similar to those used by manifold letter-writers, and is also connected with the lever, H H, and directly over the centre of the metallic roller, T, in which a slight groove is made to correspond with the point O. R represents the standard in which the axis of the roller, T, freely revolves, and is a part of D. The line, S, represents the paper, in form of a riband, passing from its coil between the roller and the point of O. N and P are the two extremities of the wire upon the magnet, A. Every part is now described; and, bearing in mind the effect of the battery, when in action, upon the soft iron, by forming a complete circuit with the wires N and P,

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the mode of writing by the instrument may be easily comprehended by what follows. Complete the circuit, and instantly the cross-bar, G, approaches the ends of the *magnet*, E, until they meet in the direction of arrow, W. Break the circuit, and G is carried up in the direction of arrow, X, by means of the spring, K. If to the roller, T, clockwork is attached, to give it a uniform movement upon its axis, the paper, S, will move with the same uniform motion under the point, O; then, by completing the circuit, the point, O, is brought down upon the paper, which is indented to such a degree as to make it perfectly apparent, and continues to mark it in that manner so long as the circuit is closed; but upon breaking the circuit the marking ceases, and the point, O, flies from the paper, which continues passing on. If the circuit is closed and broken with the utmost rapidity, then a succession of dots and spaces upon the paper appears. If the circuit is successively closed and broken with less rapidity, short lines and intervening short spaces are made. If closed for a longer time and broken in succession, then the marks become longer, so that dots, short lines, long lines, and short or long spaces, are made, according to the time the circuit is closed, and the rapidity with which the paper moves under the pen. An arbitrary arrangement of these dots, short and long spaces and lines, constitutes the telegrapic alphabet, by means of which intelligence to any extent is communicated. Thus one dot may represent A, two dots B, three dots C, one dot and a line D, &c. The paper to be imprinted is fixed upon a revolving cylinder, and records despatches day and night; and this without ink, as the impressions are easily read, even by the blind. The records of the night continue entered on the morning. The alphabet is easily learned.

Note by Editor Elec. Mag.—The following is the plan of signals employed:—

[The Telegraphic Alphabet represents each letter of the English Alphabet, with the Numerals, by which any amount of writing or correspondence may be conducted, in all the details of letters and words of the common mode of correspondence, or writing.]

Alphabet.		Numerals.	
A	– —	1	– — — –
B	— – – –	2	– – — – –
C	– – –	3	– – – — –
D	— – –	4	– – – – —
E	–	5	— — —
F	– — –	6	– – – – – –
G	— — –	7	— — – –
H	– – – –	8	— – – – –
I	– –	9	— – – —
	— – — –	0	———

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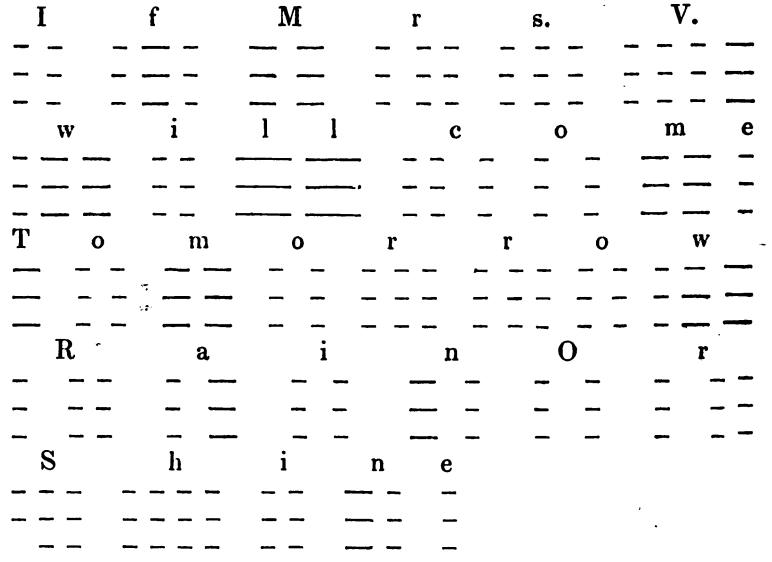
K	— - -	T	—
L	——	U	- - —
M	— —	V	- - - —
N	— -	W	- — —
O	- - - -	X	- — - -
P	- - - - -	Y	- - - -
Q	- — -	Z	- - -
R	- - -	&	- - - -
S	- - -		

NOTE.—The telegraph register strikes three copies of each character (as below), by means of three points, by which the possibility of a failure in any of the characters is avoided, which might occur if a single point only was used.

We are in possession of a slip of paper, 45 inches long, which is embossed with the following message, sent by the telegraph in its ordinary routine of working:—



If Mrs. V.* will come to-morrow, rain or shine.

* Mrs. Vail; Mr. Vail is assistant to Prof. Morse, vide p. 100.

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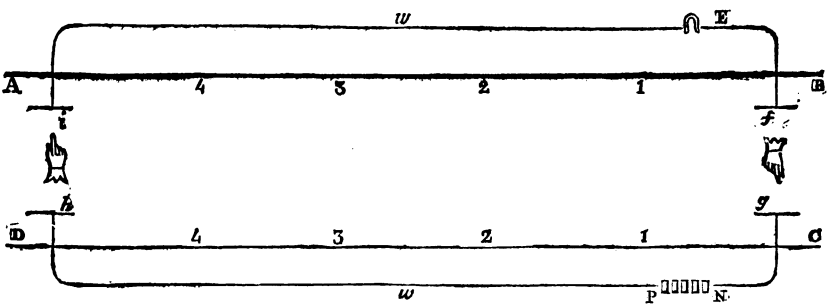
CXV. *Electro-Telegraphy across Rivers.* By Prof. Morse. From "*The Report of the United States' Commissioner of Patents,*" for the year 1844, p. 447. Extract from a letter from Professor Morse to the Secretary of the Treasury. Communicated by W. G. Lettsom, Esq.

In the autumn of 1842, at the request of the American Institute, I undertook to give to the public in New York, a demonstration of the practicability of my telegraph by connecting Governor's Island with Castle Garden, a distance of a mile; and for this purpose I laid my wires, properly insulated, beneath the water. I had scarcely begun to operate, and had received but two or three characters, when my intentions were frustrated by the accidental destruction of a part of my conductors by a vessel, which drew them up on her anchor and cut them off. In the moments of mortification, in a sleepless night, I devised a plan for avoiding such an accident in future, by so arranging my wires along the banks of the river as to cause the water itself to conduct the electricity across.

The experiment, however, was deferred till I arrived at Washington, and on December 16th, 1842, I tested my arrangement across the canal, and with success. The simple fact was then ascertained, that electricity could be made to cross a river without other conductors than the water itself; but it was not until the last autumn that I had the leisure to make a series of experiments to ascertain the law of its passage. The following diagram will serve to explain the experiment.



A, B, C, D, are the banks of the river, N, P, are the battery, E is the electro-magnet, *w*, *w*, are the wires along the banks, connecting with copper plates, *f*, *g*, *h*, *i*, which are placed in the water. When this arrangement is complete, the electricity generated by the battery passes from the positive

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pole, P, to the plate, *h*, across the river, through the water, to plate *i*, and thence, around the coil of the magnet, E, to plate, *f*, across the river again to plate, G, and thence to the other pole of the battery, N. The numbers, 1, 2, 3, and 4, indicate the distance along the bank, measured by the number of times of the distance across the river.

The distance across the canal is 80 feet; on August 24th, the following were the results of the experiment:—

No. of Experiment.	No. of cups in battery.	Length of Conductors *w*, *w*.	Degrees of motion of Galvanometer.	Size of the Copper-plates, *f*, *g*, *h*, *i*.
1	14	400	32° and 24°	5 × 2½ ft.
2	14	400	13½ ... 4½	16 × 13 in.
3	14	400	1 ... 1	6 × 5 in.
4	7	400	24 ... 13	5 × 2½ ft.
5	7	300	29 ... 21	5 × 2½ ft.
6	7	200	21½ ... 15	5 × 2½ ft.

Showing that electricity crosses the river, *and in quantity in proportion to the size of the plates in the water. The distance of the plates on the same side* of the river *from each other*, also affects the result. Having ascertained the general fact, I was desirous of discovering the best practical distance at which to place my copper plates, and not having the leisure myself, I requested my friend Professor Gale, to make the experiments for me. * * * *

As the result of these experiments, it would seem that there may be situations in which the arrangements I have made for passing electricity across the rivers may be useful, although experience alone can determine whether lofty spars, on which the wires may be suspended, erected in the rivers, may not be deemed the most practical. The experiments made were but for a short distance; in which, however, the principle was fully proved to be correct.

It has been applied, under the direction of my able assistants, Messrs. Vail and Rogers, across the Susquehannah River, at Havre-de-Grace, with complete success,—a distance of nearly a mile.

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CXVI. *On Galvanography.** By Prof. von KOBELL. From a Translation in "*Silliman's Journal*" for April, 1845, p. 221, by W. G. Lettsom, Esq.

(EXTRACTS.)

"IN order to attain a uniformity in the current, I tried what would be the effect of adding to the solution of the sulphate of copper other salts, which resist the action of such feeble currents as are here called into play, and I thus obtained a copper of the most admirable quality. The salts I thus employed were Glauber's salt, sulphate of zinc, potash, alum, and saltpetre. Chlorides are not available; for they become decomposed, and thus act on the silver on which the pictures are painted. These salts, or rather their saturated aqueous solutions, with the exception of the sulphate of zinc, take up as much sulphate of copper as common water does. I have determined the amount of oxide of copper contained in an ascertained quantity of these solutions, and have ascertained therefore, by calculation, what is the quantity of sulphate of copper which they take up. I find that 100 parts by weight of spring water dissolve about 27 parts of sulphate of copper; 100 parts of a solution of Glauber's salt 27·5; 100 parts of a solution of potash alum, 27·4; 100 parts of nitrate of potash, 25·4; but 100 parts of sulphate of zinc, only 7 parts of sulphate of copper.

"As a precipitation fluid, there are two mixtures which I usually employ,—the first consisting of two parts, by measure, of a saturated aqueous solution of sulphate of copper, mixed with one part, by measure, of a similar solution of the sulphate in a solution of Glauber's salt; the second consists of the same quantity of the solution of sulphate of copper, with the addition of one part, by measure, of a solution of the sulphate in a solution of sulphate of zinc. There are, however, other proportions; for instance, equal parts, by measure, of the various solutions which afford satisfactory results; indeed, a simple solution of sulphate of copper, in a solution of Glauber's salt, will afford an extremely malleable copper, which, when as much as half a line thick, is perfectly flexible, and presents a very even surface."

* * * * *

"Occasionally, however, the two copper plates, thus prepared, adhere together so firmly that there is no possibility of separating them; but this difficulty is altogether obviated

* This is the notice referred to by Von Kobell, in the paper given in our last, p. 3.—*Ed. Elec. Mag.*

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by the following process, which was first communicated by me to the Academy of Sciences, at their meeting of the 11th December, 1841.

"If we form a concentrated solution of common salt, and dissolve chloride of silver in it (this is done by adding thereto a suitable quantity of a solution of nitrate of silver, shaking the precipitate well up in the fluid, and then letting it settle), and into this liquor dip a clean strip of copper, it will be observed that, in from ten to fifteen minutes, there forms upon the surface thereof an infinitely thin film of firmly adhering metallic silver, so thin that on washing the plate and drying it with a towel, and rubbing it with a piece of wash leather, it presents the appearance of copper again.

"A similar coating of platina may be obtained by adding to a similar solution of salt (till the liquor assumes a pale wine-yellow colour) a few drops of a solution of platina, as neutral as possible. About one-eighth of an ounce of the solution of platina is required for a pound of the solution of salt. It takes about two hours for the film of platina to form completely upon the copper plate; but it adheres as well as the silver, if not better. In order to clean properly the plates thus coated, they are to be laid for five or six minutes in dilute muriatic acid, and then washed in warm water. If, when thus washed, they assume a bluish or yellowish hue, they must be again dipped in the diluted muriatic acid, and washed in cold water. The deposition of this film of silver or of platina is very materially influenced by the quality and the nature of the copper surface; but it is precisely upon this galvanic copper, owing to its purity, that these films are thrown down in the most perfect state; and, as they form most readily upon smooth surfaces, they prevent the plates from growing together at the very spot where their tendency to adhere is the greatest. Anything approaching a modification in the copy is out of the question by this process; for, strictly speaking, these films are not coats upon the copper, but rather they replace a portion of that metal from the surface of the plate, inasmuch as there is an interchange between the atoms of platina or silver in solution, and those of the copper. * * * * * *

"From the power of the currents depends the facility with which the plates may be separated, when the silver or platina is used, or whether, when neither of those substances are employed, the plates can be parted at all, or not."

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CXVII. *Note on* MM. DECHAUD *and* GAULTIER DE CLAUBRY'S *Processes for the Extraction of Copper from its Ores by Electric Action.* From "*Comptes Rendus*," June 2, 1845. Vol. xx., No. 22, p. 1659.

(TRANSLATION.)

THE beautiful labours of M. Becquérel upon chemical actions determined under the influence of feeble electric currents, have opened a new way, which is destined to furnish the metallurgical arts with results of which it is impossible at present to see all the importance.

With a view of making these actions serve for the extraction of copper from its ores, MM. Dechaud and Gaultier de Claubry have devoted themselves to long researches, which they believe to have become worthy of drawing attention, and to be destined to determine a complete transformation in the metallurgy of copper. These results, reduced to their most simple expression, may be put forth very briefly:—

The extraction of copper from pyritic minerals is divided into two entirely distinct series of operations: roasting and the precipitation of copper.

Roasting.—The operation is carried on in a reverberating furnace, whether in transforming directly the sulphuret into sulphate by the simple action of the air, or in profiting by a reaction, which furnishes useful results, and which consists in the transformation of oxide of copper into sulphate, by its calcination with sulphate of iron, at a dull red temperature under the influence of a current of air, the iron remaining in the state of oxide.

Proper washings extract the sulphate of copper, which retains neither arsenic nor antimony, so that very impure ores, as the *fahlerz*, furnish as pure copper as the carbonates or oxides of copper, that contain no foreign metal.

Precipitation.—The precipitation of copper from its solutions of the sulphate requires, in galvanoplastic processes, galvanic apparatus, the cost of which would prevent all metallurgical operations. So that the authors have felt it incumbent on them to turn their attention to means of realizing the same effects, by suppressing exterior piles. The following are the data, on which their apparatus are based:—

If we place upon each other, two solutions, the one of sulphate of copper more dense, and the other of sulphate of iron less dense; and in the former, we place a plate of metal forming the cathode, and in the sulphate of iron a piece of cast iron, and these two metals be united by a conductor, the precipitation of copper immediately commences; and is com-

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pleted in a longer or shorter space of time, depending on the temperature, or the concentration of the liquids, and on the extent of the metal surfaces; but, as M. Becquérel observes, the physical state of the copper presents great differences, in proportion as the solution is impoverished. This serious inconvenience is obviated by profiting by the following observations: — that, after a few instants of action, there exist four zones of liquid; beginning from the bottom of the vessel, a solution of sulphate of copper more dense, the same salt less dense, in consequence of the precipitation of a portion of the metal, sulphate of iron more dense by the dissolution of the cast iron, and the same salt less dense.

If, at the level of each of these zones, convenient openings are arranged, to introduce and to draw off the liquids in proportion as the chemical action goes on, the liquids are easily preserved in uniform states of density, and the copper is at once pure and in the same physical condition.

In respect to the application of this process to metallurgy, the extent of exposed surface necessary for the precipitation of a great quantity of copper is a very important element: on taking this into consideration it is easy to modify the form of the apparatus, still retaining the same principle.

For this purpose, instead of placing the liquids in horizontal strata, they are arranged vertically, separated by a diaphragm very permeable to the electric current, and very little so to the liquids; pasteboard fulfils these conditions perfectly, it serves for entire months, without experiencing any alteration, and the quantity of sulphate of iron, that penetrates into the sulphate of copper, is even then not sufficient to prevent the continuation of the operation; the apparatus, therefore, is arranged as follows:—

A wooden box, lined with lead, or protected by proper varnish, contains the sulphate of iron. An opening above introduces the liquid at a given degree of density, and another below permits the concentrated solution to pass away.

Into this box are immersed cases formed of a frame, the ends and bottom of which are of sheet lead (*tole plombée*, Fr.), and the lateral sides of which are furnished with a sheet of pasteboard. A lower opening gives entrance to the saturated solution of sulphate of copper, and another opening, higher up, gives exit to the exhausted sulphate. In each case is placed a sheet of lead; between them, and also on the outside of the two extreme cases, are plates of cast iron; distinct conductors, affixed to each plate, make it communicate with a common conductor placed outside the apparatus. Two reservoirs, having their levels constant, receive the two

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solutions that are to be introduced, and constantly furnish a supply. The density of the liquids is regulated once for all, and the apparatus goes on working for months together, without requiring any kind of care; the most suitable densities for the solution of sulphate of copper that escapes from the apparatus are from a quarter to a half of the saturated solution. The copper is precipitated on both surfaces of the metal plate that forms the cathode.

As the pasteboard partitions prevent the immediate contact of the two liquids, it is re-established by making small holes in the latter above the upper edge of the plates of metal that form the cathode, which permits the solution of sulphate of iron to superpose itself upon that of the sulphate of copper; the vertical apparatus then fulfils the same conditions as the horizontal apparatus.

At a temperature of 20° C. (66° F.), a square metre (1·19 sq. yd.) of surface receives 1 k. g. (2·2 lbs.) of copper in twenty-four hours.

The precipitated copper is pure, and is constant in its physical character; the sheets may be immediately wrought with the hammer, or passed through the flatting machine; in this case, four or five passes bring the metal to the density of 895; and thus are avoided all the operations necessary for making it pass from the state of blocks to that of sheets. The work presents no difficulty; no refining is required; no dross is given. In a regular fabrication, as much as 75 per cent. of sheets are obtained; the rest of the precipitated copper is partly in pure fragments and partly in powder of cementation. The authors calculate in the metallurgic operations for at least 50 per cent. of sheets, and 25 per cent. of divided copper, which only requires fusion to be brought to the state of lumps or ingots, and 25 per cent. of divided copper which would require fining.

The question relative to the application of galvanic action to the metallurgy of copper thus appears to be reduced to a state of simplicity almost as complete as possible. It is scarcely necessary to say that, by this process, galvanoplastic objects may be easily obtained on a large scale.*—[A.]

CXVIII. *Improvements in Obtaining Copper from Ores.* By W. H. Ritchie. Extracts from a Patent *sealed* Oct. 10th, 1844; *enrolled* April 10th, 1845. Vide "Repertory of Patent Inventions." June, 1845.

"Many attempts have heretofore been made to obtain copper

* A communication of this process has been patented in England. We give a few extracts from the specification in Article CXVIII.—*Ed. Elec. Mag.*

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from the copper ores by dissolving them, and then precipitating the copper on to iron or other metal by galvanic currents; but in such cases the exciting liquid and the solution of copper have been separated one from the other. Now one part of the invention consists in using an exciting liquor in contact with the solution of copper from which the copper is to be precipitated * * * *

"The calcined copper ore is then to be dissolved, as is well understood, and such solution is placed in any suitable vessel, and I prefer them to be rectangular, of any convenient length; and on the upper surface thereof I place a solution of the sulphate of iron, to be used as the exciting solution; and, thus prepared, I introduce what I call the generating plate, or surface of cast iron, which is connected to [by ?] copper, or other conducting material, with a plate of lead or suitable metal, having an equal extent of surface with the cast iron generating surface; and these plates or surfaces so connected, being introduced into solutions in the vessels, the copper in the solution will be quickly deposited, &c. * * *

"It will be found that, in the course of working, the solution of the sulphate of copper, which at starting is a saturated solution, will become at its upper part lighter than lower down; and I prefer that it should be drawn off when it has lost about half its copper; and it is evident that the solution of sulphate of iron, which at starting I make by mixing two parts of water with one of saturated solution, will become at its lower part of greater specific gravity than the upper part; and I prefer to draw off the solution, when it becomes as dense as the weaker solution which is being drawn off."

CXIX. *On the Action of Magnetism upon all Bodies.* By M. Ed. Becquerel. From "*Comptes Rendus,*" June 9, 1845. Vol. xx., No. 23, p. 1708.

(Translation.)

From the results contained in the memoir that I have the honour of presenting to the Academy, the following conclusions may be deduced:—

1st. When cylindrical and solid bars of soft iron, of the same length but of different diameters, oscillate under the influence of a magnet, the cube of the times of the oscillations

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is proportional to the weight of the bar, or rather to the square of the diameter.

This law involves the following:—

2nd. When cylindrical bars of soft iron, of the same length but of different diameters, are subjected to the influence of a magnetic bar, they are momentarily magnetized, in such sort that the magnetic intensity of each bar, or the sum of the molecular magnetic actions, is proportional to the cube root of the weight, or cube root of the square of the diameter. It follows from this, that the magnetic action exercised on each elementary fibre of the bars is the more feeble as the latter is larger; and this action is in inverse ratio to two-thirds the power of the weight.

3rd. When hollow cylindrical bars are exposed to the influence of a magnet at the same time with solid cylindrical bars of the same weight, the magnetic action exercised on each elementary fibre of the former is stronger than that which occurs on each fibre of the latter.

4th. When a magnet is placed at a certain distance from a bar, and the magnetic density of the latter on the quantity of particles of iron comprised under the same volume varies; then, if this density is greater than one-tenth, the elementary action of the magnet is proportional to the square of the magnetic density of the bar. If this density is much more feeble, and the particles of iron are very far apart, their action is proportional to the magnetic density.

The second part of the preceding law was established by Coulomb.

5th. The action exercised on the part of a magnet upon iron is always the same, whether the metal be in the malleable state, in the state of filings, or even of the impalpable powder reduced by hydrogen, if we regard the magnetic intensity of the matter.

6th. At the ordinary temperature, the specific magnetism of soft malleable nickel is the same as that of soft iron; that is to say, that two similar bars of soft iron and soft nickel, of the same length and the same weight, oscillate in the same time under the influence of a magnet.

7th. From the considerations put forth in this memoir, although we have not been able to procure soft malleable cobalt, it is probable that its specific magnetism at the ordinary temperature, is the same as that of soft iron or nickel.

8th. The coercitative force of cobalt increases much more than that of the two other magnetic metals, when, while in a spongy state, it is subjected to compression or is struck.

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9th. Nickel loses its magnetic faculty at about 400°, iron at cherry red, and cobalt at the red-white of a forge.

10th. The specific magnetism of soft iron varies but very little between the ordinary temperature and that of dull red; only at dull red it increases about $\frac{4}{100}$, which shows that, at the ordinary temperature, this metal behaves as if it had a feeble coercitative force.

11th. The specific magnetism of cast iron and steel increases with the temperature, in such sort that, before being annihilated in the metal at cherry-red, it is equal to that of soft iron.

At nascent red the specific magnetism of cast iron is at its maximum.

12th. Nickel, cobalt, and their carburets, behave in the same way as iron and its carburets. Thus cast iron at a dull red, cast nickel before 400°, and cast cobalt at cherry-red, have the same specific magnetism as soft iron, cobalt, and nickel.

These experiments were made by a new method of observation, by suspending the bars from a torsion platinum wire, in a stirrup of the same metal, and exposing the whole to the proper temperature, and making the bars oscillate under the influence of magnets.

13th. The specific magnetism of the natural magnet (oxidulated iron) becomes null before the red temperature. It increases from the ordinary temperature up to this limit. At the ordinary temperature, the specific magnetism of crystallized oxidulated iron is 0·48, that of iron being 100.

The action of heat on the three magnetic metals may be expressed by the following table:—

Temperature.	Nickel.		Iron.			Cobalt.	
	Malleable.	Cast and Carburetted.	Malleable.	Cast and Carburetted.	Steel.	Malleable.	Cast and Carburetted.
Ordinary temp.	100	62 variable.	100	80	variable.	100?	41 variable.
Below 400°	Iron, Nickel, and Cast Nickel, have the same action.						
About 400°	0	0					
Nascent red			104	109 maxim.		104?	107 maxim.
Dull red			104	104	104	104?	
Cherry red			0	0	0	104?	104
Bright red of the forge fire						0	0

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The numbers given for malleable cobalt are hypothetical; but, from the analogies, one is led to suppose that they are such as I have indicated.

14th. The specific magnetism of iron being 1,000,000, that of the specimens of chromium and manganese which I was able to procure is 258 for the chromium, and 1137 for the manganese. On account of such low numbers, we cannot decide whether or not this action is due to a mixture of iron.

15th. All natural bodies obey the action of magnetism, as Coulomb first observed; and principally organic matters, rocks, and crystalline minerals.

16th. This action never remains the same for the same substance, and it varies with its purity. In proportion as silica, iodine, and camphor, are purified, this action diminishes, and is completely annihilated in certain specimens.

17th. The fraction of iron necessary for the production of these phenomena would not be the millionth part of the weight of the needle subjected to experiment, for the purest specimens. From this there are probabilities that these effects are due to mixtures of ferruginous or magnetic matters.

I have given in a table the specific magnetism of some of the principal rocks that compose the crust of the globe.—[A.]

CXX. *On the Liberation of Electricity in the act of Vegetation.* M. Becquerel. "Eléments d'Electro-Chimie," p. 305, 1843.

(Translation.)

Before setting forth the changes that are brought about in organised bodies and organic substances, when subjected to the action of electricity, we think it necessary to point out the means by the aid of which we succeed in recognising, in all these bodies, the presence of electricity, which from effect may frequently become cause. But in the first place, we must call to mind the principle that has been laid down, and of which the following is the expression:—Every chemical action gives rise to a liberation of electricity subjected to laws such that, in combinations, acid sets positive electricity free, and alkali the contrary electricity. In decompositions, the inverse occurs. Let us make an application of this to vegetables:—The liquids by which the tissues are moistened, are not always homogeneous. The sap, which is not evaporated by the action of the air in the leaves, does not enjoy the same properties as

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that which is elaborated; separated by a membrane, there result electric currents, which may be concerned in the phenomena of life. Oily and gummy liquids, which are not conductors, do not partake of this property, at least in a degree appreciable by our apparatus. If we plunge the two extremities of a platinum wire into a fruit (an apple or a pear), one on the side of the stalk, and the other on the side of the eye, a tolerably strong deviation is produced by the electric current, the direction of which indicates that the stalk acts as an alkali and the eye as an acid. The peach, the apricot, and the plum, give effects in a contrary direction. There is a want of all current when the two extremities of the wire are immersed at an equal distance from the centre of the fruit, perpendicularly to the plane which passes through the eye and the stalk. These currents do not indicate the presence of an acid or an alkali, but rather that of substances which, in reacting on each other, behave as such. If we cut a fruit into two parts, and express the juice of each, and then place it in two separate vessels, communicating by means of a slip of paper, precisely similar effects are obtained.

It is probable that there must exist a multiplicity of electric currents, circulating in the tissues, by their intervention, and upon which the products must be deposited. See, for example, the electric currents produced by the reaction of heterogeneous solutions; it remains, therefore, to see whether we can really obtain free electricity in the different acts of vegetation. It is one of the most complex questions, on account of the great number of elaborations that occur.

Let us see what passes in the act of vegetation. During the day, carbonic acid is decomposed; during the night, on the contrary, a part of the carbon accumulated during the day, combines with the oxygen, which the green parts absorb; there are also other secondary elaborations, which concur in the act of vegetation; all these actions liberate electricity. When there is formation of carbonic acid the oxygen takes positive electricity, and the carbon negative electricity; the effects are the reverse when this acid is decomposed; but the electricities that are liberated recombine when the two bodies are not expelled; it is the same when the decomposition takes place very slowly. There are so many causes concurring in the vital functions, that it is a very difficult matter to anticipate, *à priori*, what may be the result of the electrical effects produced.

The question is less complex when one of the bodies is expelled from the vegetable. M. Pouillet made the following

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experiment: he placed in two rows a dozen capsules of varnished glass, resting on a table likewise varnished; they were filled with mould, and placed in connection together, and also in communication with the upper plate of a condenser, by means of metal wires, whilst the lower plate was in connection with the earth. Seeds were sown; the first two days they were swelling, and there was not any electrical effect: when the plantules escaped from the earth, the gold plates possessed an excess of positive electricity; hence the capsules had acquired negative electricity, and the escaping gases positive electricity. Although the chemical effects were in inverse order, the same phenomena were produced during the night. The seeds experimented upon were corn, nasturtium, gilliflower of Mahon, and lucerne. At the end of three or four days, a single second was sufficient to charge the condenser. In order to succeed, it is necessary to operate in dry weather, and in a dry situation, a condition which is obtained by scattering lime in the apartment.

In order to interpret this result, we must call to mind that, in germination, a veritable combustion occurs; hence the electrical effects must be the same; in the first periods, as the gas which is formed remains enclosed in the earth, there must be a recomposition of the two electricities that are liberated. There must be an escape of carbonic acid gas, in order that the condenser may become charged. When the work of germination is completed, how comes it that the electric effects are the same by day and by night? It is a difficult matter to reply to this question. The evaporation may in like manner give rise to electrical effects; for the water must sometimes carry away with it organic principles. We are, therefore, really in doubt as to the true cause of the liberation of electricity in the preceding experiment. If, in addition to this, we remark that organic substances, which are found in the earth, in like manner experience a veritable combustion, whence must result electrical effects, similar to those which take place in germination, it follows that the effects obtained by M. Pouillet, have not a very certain origin; and that it is a very difficult matter to study the liberation of electricity in vegetation.

We may ask whether there exist in vegetables composed of heterogeneous parts, currents resulting from the continual reaction of these parts upon each other? We may reply affirmatively to this question, by resting chiefly on the following experiment of Dr. Bacomio of Milan: we take discs of beet-root, and discs of walnut tree, deprived of the resinous principle, by digestion in solution of cream of tartar in vinegar. We form a

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pile of them; and if we take as a conductor two leaves of scurvy-grass, we obtain contractions in the frog. It is, therefore, manifest that with organic substances alone, a pile may be formed, and currents may, consequently, be produced.—[A.]

CXXI. *Observations on Electro-culture, with details of an Experiment illustrating the Influence of Electric Currents on the Economy of Vegetation.* By W. H. Weekes, Esq.

The subject of electro-culture has at length, by a sudden and convulsive effort, as it were, attracted the attention of various experimentalists; and I learn with satisfaction that, not only men of research, but the agriculturist and the horticulturist are practically uniting to test the influence of electricity in its several forms, as applicable to the economy of vegetable life, in divers parts of the kingdom. This is exactly as it should be. We do not want *arguments* to show that electricity can or cannot be turned to the account of vegetation; it is *facts* alone that we need in the first place, such as are derived from experiments based upon legitimate premises, and these results of individual inquiry, numerously and honestly recorded; after this we may more consistently afford to employ a little time and ingenuity in the form of arguments.

As regards the very interesting and important subject of electro-culture, I am entitled to state that it is one by no means new to me, my attention having been devoted thereto, by occasional series of experiments variously designed, through a period of about seventeen years; and I add with pleasure that many of the trials alluded to were undertaken in conjunction with the labours and attentive observations of my friend Mr. Thomas Pine, of Maidstone, whose interesting papers on electro-vegetation will be found in the "Proceedings of the London Electrical Society," to which society they were communicated by the honorary secretary, C. V. Walker, Esq.

I perceive that my friend Mr. Noad, has, in a recent edition of his excellent work entitled "Lectures on Electricity," &c., obligingly introduced several notices of my early inquiries and experiments on electro-vegetation; and at page 74, of his book occurs a brief mention of one of my first trials in electro-culture, by means of a voltaic current derived from a battery of thirty plates, charged with salt and water. This experi-

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ment, with many others of a similar nature, was made in the year 1834; and I recur to it now only for the sake of adding that my friend Noad's information on the subject did not enable him to state, that not only was the flower-pot mentioned by him "insulated" and kept, as it were, "in an electrified atmosphere" *occasionally*, but a voltaic current passed almost constantly through the mould in which the seeds vegetated. It appears that his own experiments "failed to show any advantage in favour of electrified soil," although they seem to have been conducted with his usual share of skill and attention. I am thoroughly satisfied with the accuracy of his report, and feel, moreover, that I am incompetent to assign a reason for his failure, yet I am persuaded that he would by future trials, and, perhaps, somewhat varied conditions, ultimately arrive at results similar to that obtained in several of my former experiments, although I cannot regard my manipulations at that period as having been, by any means, such as were best suited to the occasion.

Much contention, and I fear some acrimony, at present manifests itself in connection with this inquiry. While one experimentalist calls public attention to successes in electro-culture little short of miraculous, from the employment of a curiously constructed apparatus, setting at defiance all established laws of the science, and which the practised eye of an electrician at once perceives to have been nearly, if not wholly useless;* another party denounces as visionary even the best directed efforts, and sweepingly declares, "that man cannot, by any of his paltry efforts, give to vegetation more of the electro-vital principle than nature has already taken good care to supply."† Perhaps it might, with nearly the same propriety, be asserted that nature had given to vapours, as they spontaneously rise from the earth's surface, all the mechanical force that *man by his paltry efforts* is capable of developing from steam. Nature, we know, will not submit to have conditions prescribed for her, although she disdains not to couple her labours with art, and seldom refuses a favourable response when fairly interrogated. Truth is seldom found on examination to reside in the extremes of opinion, and this general rule receives support in the present instance; but facts indisputably show that the electro-vital principle in vegetation is capable of being greatly promoted, both by means of free electricity skilfully drawn from the atmosphere, and also by

* Vide *Elec. Mag.* No. 9, Vol. II. p. 47.

† See report of Professor Bachhoffner's lecture on Electro-Culture, in "The Critic" of July 19th, 1845.

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well-directed voltaic currents through the soil and otherwise, although, I have reason to believe, less so in the former than in the latter case.

In the beginning of the present year I resolved on renewing my experiments on electro-culture, by means of battery currents of varied tension, some of which have been sent constantly through the soil occupied by growing plants sought to be thus influenced, and the operation of others confined to portions of individual vegetable systems. I could narrate several examples of complete success, as well as others of partial or entire failure from inefficient means and incidental causes, but I shall select the following experiment because its progress has been witnessed by the greatest number of attentive observers, among whom are to be found those remarkable for acuteness of judgment, as well as for their high scientific attainments.

The battery employed in this experiment consists of a single pair of cylinders, the zinc or positive metal $3\frac{1}{8}$ inches in diameter and 6 inches in depth, surrounding, at the distance of a quarter of an inch, the negative metal, which is of iron, 3 inches in diameter, and, for the convenience of carrying off the conducting wire, standing half an inch higher than the positive cylinder. Both metals are cut from the usual sheet forms, commonly known among manufacturers. This voltaic pair, imbedded in finely sifted earth, is placed vertically in the centre of a coarse, glazed, earthenware jar, capable of holding rather more than two quarts, resting conveniently upon a shelf placed near the origin of the branches of a small apple tree, further support being given to the apparatus by a stake driven into the ground, as shown in fig. 1, which also conveys an exact representation of the skeleton tree at the commencement of the experiment.

The tree selected for the occasion is a dwarf, consisting of two branches, both of which are grafted with Deventon pippins, the grafts having been twice headed down; during the last season it produced twelve tolerably strong and nearly equal shoots. The remarkable backwardness of vegetation incident generally to the spring of this year, caused the application of a voltaic current to be delayed much beyond what was at first intended, but on the 28th of April, the apparatus having been fixed agreeably to design, a copper wire, *a*, connected with the positive cylinder, was plunged about half an inch into the centre of a spur projecting from the lower part of the shoot, *b*, a small portion of the spur having been cut off transversely to facilitate this operation. At the top of the shoot, *b*, by no means one of the largest, a similar portion was

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Fig. 1.

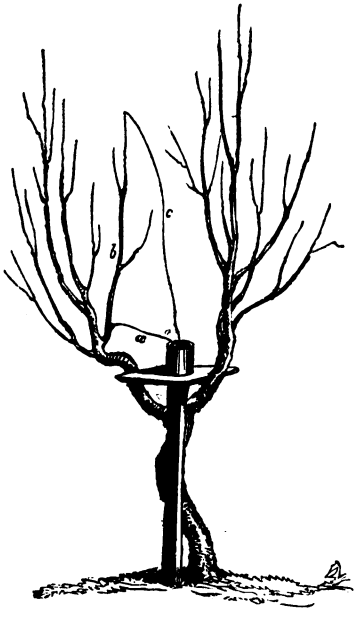
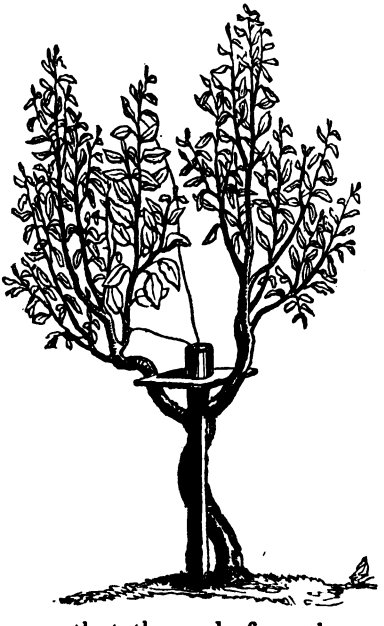


Fig. 2.



removed by a transverse cut also, so that the end of another copper wire, *c*, proceeding from the negative cylinder, could be made to enter the pith at this extremity of the young shoot. From the date above mentioned, the mould within the jar in which the cylinders are imbedded has been regularly kept moistened by means of a weak solution of salt in spring water, and a constant current of low tension has been traversing the shoot *b*. It somewhat unfortunately happened that no blossoms made their appearance, and nothing remarkable was evident until a short time after the development of foliage, which took place pretty uniformly over all the shoots. Soon, however, the leaves proceeding from the electrified shoot presented the most characteristic and decided superiority over the rest of the tree, being not only much larger, but of a deep green colour, and fine glossy appearance. About this time the disease called *canker* made its appearance *on all the shoots*, and it was deemed necessary by the proprietor that they should be again headed down, with the exception of the one through which the electric current is still passing. Notwithstanding this prudential measure, the ravages of the disease have continued upon every shoot, save the one under electrical influence, and the bark upon *that* shoot has recently

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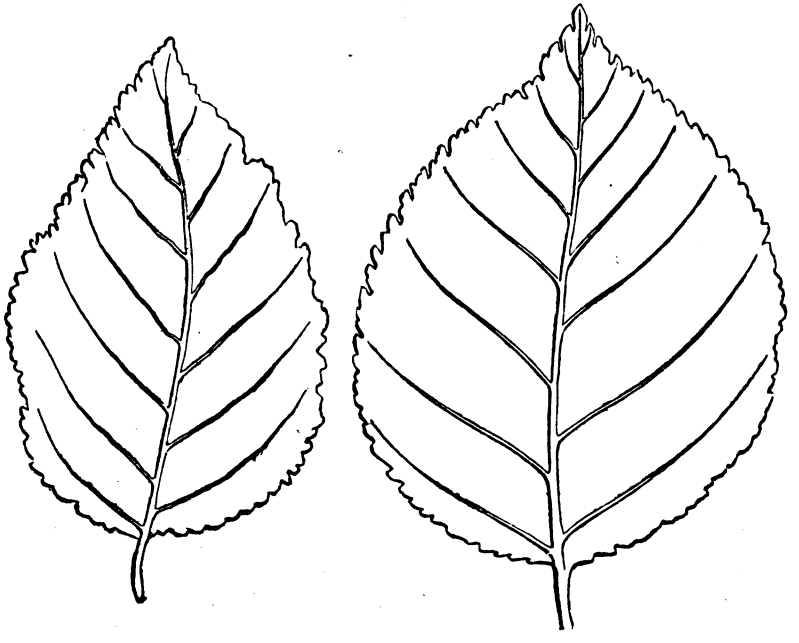
returned to a sleek and healthy condition; and while the foliage of the tree generally has become shrivelled, brown, and rusty, that growing on the electrified shoot, to speak correctly, maintains a most vigorous appearance. No words can so well convey a notion of the present state of the tree, as the excellent sketch fig. 2, which was made on the spot by a friend only a few days since. In fig. 4 is represented a leaf of average

Fig. 3. Fig. 4.



natural size, grown on the electrified shoot, and in fig. 3, one of the average leaves from the other part of the tree, forming the subject of this experiment.*

It may still very properly remain a question how far electrical agency can be rendered available in the extensive operations of the agriculturist; but I conceive it would be only the madness of scepticism that would deny the influence of electric currents on the vegetable economy, in the face of an open experiment attended with such decided results, as those I have now the satisfaction to put on record.

* Mr. Weekes forwarded specimens of the *largest* and *smallest* leaves of the electrified shoot, and also the *largest* leaf that could be gathered from the unelectrified shoot. The difference was very great. The sketches given of the "average" leaves, are engraved from the leaves themselves.—Ed. *Elec. Mag.*

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I have only to add my unqualified thanks to Mr. T. Baker, junior, of Moatsole, Sandwich, in whose garden the experiment is still in progress, and who has obligingly lent me every assistance on the occasion.

Sandwich, 2nd August, 1845.

CXXII. *On the Electricity of Mineral Veins.* By Robert Hunt, Esq.

To the Editor of "The Electrical Magazine."

Sir,—In a recent number* of your Journal you have favoured the English scientific public with a translation of a memoir by M. Becquérel, in which that distinguished experimentalist refers to certain experiments made in Cornwall, on the electricity of mineral veins; and, without mentioning any names, he at once declares that these experiments are of a doubtful character, owing to the plans adopted by the experimentalists. M. Becquérel then describes the method, as if, in making the connections between the copper-wire employed and the copper-ore of the lode, iron-nails had been used, which he says, correctly enough, would be sufficient of themselves to set up a current which would affect a delicate galvanometer. I do not know to whose experiments M. Becquérel alludes. I am not aware that they have, in Cornwall, been made by more than four or five individuals; Mr. R. W. Fox; Mr. W. J. Henwood, now at Gongo-Socco; Mr. J. Phillips, now of the School of Mines, at Paris; and Mr. Phillips, of Tucking-mill, in conjunction with myself. With respect to all, except Mr. Henwood's, I can safely declare that no such plan as the above was ever adopted in any of the experiments; and my firm impression is, that it was never adopted by Mr. Henwood. Mr. Robert Were Fox, who was the first to direct attention to the fact that electrical currents traversed metalliferous veins, and in all probability played a most important part in the formation and subsequent changes of the ores themselves, invariably adopted the greatest precautionary measures to prevent any deceptive results from local chemical action. As it was under the advice of this gentleman that all the other experiments have been made, his methods have been adopted, and many additional precautions taken to ob-

* Vide Vol. I., p. 570.

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viate the difficulties which he had himself experienced and guarded against. It may, however, be more satisfactory to state, in as few words as possible, the mode of experimenting adopted by myself.

The apparatus employed consisted of two coils of stout copper-wire, one consisting of about two hundred yards and the other of about twenty yards; and two galvanometers, one of them a very delicate one with astatic needles, the other consisting of one heavy needle and about a dozen coils of wire, being by no means of a very sensitive character. In addition to these, various mechanical contrivances, as screws, clamps, &c., for the purpose of insuring perfect connection between the ore and the copper-wire, were used, these being invariably made of copper. Plates of platina and of zinc were employed, not for the purpose of making connection, but with a view of ascertaining if the galvanic current produced by the dissimilar metals overcame, or in any way interfered with the native current of electricity. They were used in fact for ascertaining if the electricity of the mineral lode was due to chemical action between the two,—the ore and copper-wire,—or otherwise.

The conditions under which the experiments were tried were as follow:—The metalliferous veins of Cornwall are found, in general, in the immediate proximity of the junction of the granite and the killas, or clay-slate formations. They have mostly an east and west direction, although, in some districts, the lodes are found running nearly north and south. They maintain a remarkable parallelism with the "courses" or veins of Elvan, which are found inter-penetrating both the granite and the slate; but are mostly bisected at nearly right angles by the faults which extend across the country.

Having selected a lode tolerably productive in copper-ore, and of which a considerable portion was exposed to view, one wire was connected by breaking out some fragments of ore and jamming the copper-wire into the hole as firmly as possible. The other end was then connected with binding screws, or by mercury cups, with the galvanometer. One end of the second coil of wire being connected with the other pole of the instrument, the assistant carried the other end along the "level," and constantly pressed it against the rocks and other parts of the lode, whilst the galvanometer was attentively watched. In no instance was any deflection of the needle evident, when connection was made between any two parts of the same lode, unless there was a difference in the character of the ore it contained; and I never could detect any current passing between the lode itself and the rocks in which it was enclosed.

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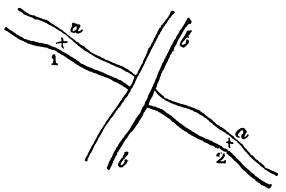
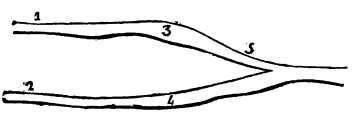


If, however, the metalliferous veins are divided by a fault or "cross course," as in the accompanying figure, where *a a* represents the vein, and *b b* the cross course; and connection is made on either side of the fault, as at 1 and 2, very decided currents pass through the wires, and consequently produce large deflections of the needle of the galvanometer. In East Pool mine, near Camborne, under conditions similar to those given in the figure, the current was so great that the large and sluggish needle was spun round with much violence, and here, for the first time, I succeeded in decomposing iodide of potassium by the current. It was found that the contact of a mere point with the ore was sufficient; and, when a platina or a zinc plate was interposed between the ore and the wire, it was found that, although the current was partially interrupted, its direction was never altered, which it would have been had the current been due to chemical action induced by contact.

At East Wheat Crofty mine, two mineral lodes run for some distance nearly parallel to each other, and eventually join, thus.—The wires being connected with any of the points numbered — that is, between 2, and either 1, 3, or 5; or between 4, and either 1, 3, or 5, powerful currents were detected, but none between 4 and 2, or 1, 3, 5; the lode being in these cases as good a conductor as the wire; but where the lodes ran in together, the metalliferous continuity was interrupted by large formations of quartz crystals. There, as in the other cases, chemical decomposition was effected.



Since that period, at Pennance mine, near Falmouth, Mr. Robert Were Fox has succeeded in procuring a very perfect electrotype copy of an engraved plate,* by the decomposing power of the electrical current passing through copper-wires connected with two lodes, one of sulphuret of lead, and the other of iron-pyrites, at some distance apart.

It will be seen, I hope, from this statement, that M. Becquérel has not correctly stated the circumstances of the experiments tried in the mines of Cornwall. That the electricity is not all due to the contact of bodies, dissimilar as sulphuret of copper and metallic copper are, I am perfectly

* Vide *Elec. Mag.*, Vol. I., p. 191.

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satisfied; but I must at the same time confess, that, as it regards the question whether the electric currents traversing the metalliferous veins are the result of chemical action between the waters percolating them and the ores they contain, or whether they proceed from an independent source, I feel the greatest degree of uncertainty. Great chemical changes are constantly going on in these metalliferous masses, the cause to which these changes are to be referred is at present, to us, merely conjectural; and on this point we require a considerable amount of experimental evidence.

I fear I have already occupied too large a portion of your journal; I therefore conclude this explanation, which will, I hope, place the question in a clear view.

I am, Sir, yours obediently,

ROBERT HUNT.

London, August 12th, 1845.

CXXIII. *On the Electricity of Paper Mills; and an Analysis of Friction as an Exciting Cause.* By CHARLES V. WALKER.

BY referring to Mr. Armstrong's and to Dr. Hankel's papers on this subject, given in pages 459 and 501 of the first volume of *The Electrical Magazine,* it will be seen that the cause of the electricity in question has not been satisfactorily determined. I may, therefore, be pardoned in giving the conclusions to which I had indeed inclined on first reading the accounts, and which have been confirmed by personal observation. On July 17th, in company with Mr. Pollock and others, I visited Mr. Elliot's mill, near Chesham, Bucks, referred to in Vol. I. p. 503; and by the kindness of the proprietor was enabled to witness the several phenomena that have been so clearly described by Mr. Armstrong and Dr. Hankel. I have nothing further to add to Mr. Armstrong's account, in reference to the process generally, and the electrical phenomena attendant on it, than that the paper becomes *negatively* electrified, which I do not see noticed in his account. We determined this (which is also mentioned by Dr. Hankel) by the usual static effects, and by the decomposition of iodide of potassium.

The main fact in reference to the excitation of the paper is, that when it reaches the polishing rollers in a physically dry state from the heated rollers, it is un-electrical; but after it

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has passed the polishing rollers it becomes electrical; and the more the paper accumulates on the final wooden roller, the larger spark will the roll give. We tried it when a fresh length was commenced, and obtained it very feebly; we tried it when the length was 3000 ft. and obtained sparks from two to three inches long. The paper then in hand was that employed by paper-stainers; it accumulated at the rate of 28 ft. per minute. We were informed by Mr. Elliot that the paper which gives the greatest effects is a thin brown paper, manufactured for Terry's "Poor Man's Plaster;" this gives the sparks of a foot or more in length, spoken of by Mr. Pollock, p. 503. Mr. Elliot caused a considerable length of paper to be made in our presence, without having undergone the final act of pressure between the polishing rollers; so that we were enabled to satisfy ourselves as to the fact of its being no longer in an electrified condition. The question, therefore, resolves itself into this;—before it reaches the rollers it is in a normal state; when it leaves them it is negatively electrified. But what is the action of the rollers? By their means, a *pressure* and a *withdrawal of pressure* occurs to every part of the paper. Now, because the polishing cylinders are *rollers*, moving on their axes, we are not slow in jumping to the same conclusion as did Mr. Armstrong—"that inasmuch as the paper moves in the same direction, and with the same velocity, as the surface of the rollers, there can be *no appreciable friction in the case.*" And this conclusion is so reasonable, that, having attained to it, we are deprived of friction, which is so variously manifested as the great mechanical exciting cause, and are compelled to do our best to find some other explanation. But are we right in saying there is *no friction?* For my own part, I believe that all the elements of friction are as much present in the case before us as they are in the excitation of an electrical machine, by rotation in front of the cushions. The first book on which I lay my hand* gives the following definition: "Friction is said to be *sliding*, when the parts of one surface move parallel with the other; it is known as *rolling*, when a round body turns about the surface of some other body." But, without concerning ourselves with mere definitions, it will be more to the purpose if we rigidly analyze the two cases. The successive parts of the glass cylinder, as they arrive at the cushion, may be represented by the figures 1, 2, 3, &c.; and the successive portions of the cushion by the letters *a*, *b*, *c*, &c. We must for the moment divest ourselves of all idea of the cylinder as a whole, and so of the cushion; we must regard the almost

* Peschel's *Elements of Physics.* Longmans, 1844.

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mathematical line (1) of glass in contact with the similar line, (*a*) of the cushion; the glass revolves; the pressure between 1 and *a* ceases; and 1 now presses on *b*, and then on *c*, and so on; in like manner 2 and 3, &c., follow in due course; the pressure between 2 and *a* occurring while 1 is pressing *b*, and so throughout; in fact, a complete succession of pressures given and pressures removed; the resistance being great because many points are in simultaneous contact, and many superficial obstacles oppose this *sliding* mode of removing pressure. In the other case, the successive parts of the paper may be represented by the figures 1, 2, 3, &c., and those of the rollers by *a*, *b*, *c*, &c.; but when 1 is pressing on *a*, and the paper moves on, all pressure is at once removed from 1; 2 is then pressed on *b*, and so on; so that the only real difference is, that, in the case of the paper, each particle has but *one pressure*; in the case of the electrical machine, each particle has *a succession* of pressures, dependent on the width of the rubber. If the rubber were only a line in width, the two cases would be one.

I gave this view of friction's being a succession of contacts made and broken, in my "Treatise on Electricity," in vol. cxxxi. of the *Cabinet Cyclopædia*, published in January, 1844. It was reasonable; and it enabled me to see less discrepancy between the sources of electric and voltaic actions than is generally allowed. I called it the "Theory of Renewed Contact." I have several times purposed reprinting it in the *Electrical Magazine*, but have withdrawn it to give place to the more important labours of others. I may give it in some future number; in the mean time I would state that the *contact*, which I recognise in a voltaic pair, is that between the zinc, for instance, and the oxygen of the water; the removal of contact is the solution of the oxide in the acid water, when a fresh particle is brought into contact, and so on.

C. V. W.

August 19*th*, 1845.

CXXIV. *Notice of the Effects of a Lightning-Flash at Sandwich, in Kent.* By W. H. Weekes, Esq.

Early on the morning of the 3rd of July last, the eastern parts of the county of Kent were visited by one of the most tremendous thunder-storms which I remember to have witnessed at any time during the last twenty-four years. The principal storm-cloud, to say nothing of its widely extended appendages, came up from the south-west, and extended at one

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and the same time completely over the town of Sandwich and a wide tract of the adjacent country. In its progress towards us, one of its electrical discharges had set fire to a thatched building about six miles distant. A few minutes before four o'clock, the usual notices of such atmospheric disturbances caused me to descend to my laboratory, situate on the ground floor, and within the window of which terminates my exploring wire, stretched between the two principal churches.* The waves of electric light and torrents of dense sparks passing through the apparatus at this time were not only most magnificent, but have rarely been equalled. A few minutes after four, an awfully grand, as well as intensely brilliant flash of lightning, rushed with a whizzing noise between the terminal balls of the instrument, followed *instantly*, as it were, by a deafening crash of thunder, which seemed to shake the surrounding buildings. I have no doubt it was the same flash that produced the phenomena related in the subjoined letter of the Rev. James Layton; and, notwithstanding this gentleman has designated his valuable account of the matter an "unscientific" and "roundabout way" of telling the "tale," I shall very much prefer what he has said as an eye-witness to any description of my own, although Mr. Layton kindly invited me to a minute examination of the track pursued by the lightning early the following day. I shall only remark that the dwelling-house of Mr. L. is an ancient pile, of the Elizabethan period, standing on the north-west side of the town; and that the chimney upon which the lightning struck is one of a stack situate at the southern gable of the house.

Sandwich, July 20th, 1845.

Dear Sir,—As you think that a detailed account of what occurred in my chamber, when the thunder-storm passed over this place on the 3rd of this month, may be interesting to you, I with pleasure relate it; but must beg you to excuse the unscientific manner in which it will be done.

I was awakened by the thunder at four o'clock, consequently, it was daylight; neither the bed curtains nor those of the window were drawn; the window, moreover, is opposite the foot of the bed, about six feet from it, and the fire-place, containing a Bath stove, on the left side of the bed, not quite so far off. The foot curtains were hanging loose round the bed-posts.

I was watching the lightning, when my eye was attracted by a momentary blaze between the fire-place and the window, just beyond my bed-curtain, attended by a very loud, sharp,

* Vide *Proceedings of the Electrical Society*, p. 41.

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stunning noise,—it seemed like the discharge of a cannon; a volley of mortar fragments flew all over the room. Afterward—a second, perhaps—came the thunder.

The blaze was elliptic, about four feet horizontally, and two vertically, as perfectly distinct as the blaze of a gas light; its outline, however, was not defined like that, which is compressed by the atmosphere; while here, the pressure being sudden and from within, the border was diluted and ragged.

You may smile, perhaps, at my supposing that I could distinguish and remark a thing so simultaneously come and gone; but thought is at least as quick as lightning, and the thought was formed that I never saw lightning in that shape before, ere the report could reach my ear only seven feet off.

The consequent stench was abominable; it seemed as if driven into the substance of everything in the room. The next day it had not entirely gone by; nor could I get rid of the flavour from my throat. Nothing was burnt. The cotton curtain so close to it was not in the least changed: not a pane of glass was broken.

On examination, the electric body appeared to have entered the chimney through the side of the pot, in which it made a hole large enough to admit two fingers, and which it forced about eight inches from its place. It then descended some twenty or five-and-twenty feet, when it came through a crack in front of the chimney, and burst the paper, forcing out the canvass, without tearing it, as if the blunt end of a stick had been pushed against it. Thence it passed downwards between the bricks and the canvass till it met obstruction in the wooden mantel-shelf, fastened to the bricks, when it broke into the room, bursting both canvass and paper, and forcing the woodwork about an inch forward; fragments of the paper, as well as the mortar, were driven quite across the room.

Now I cannot say I distinguished the electric *spark;* but I saw the blaze where I have described it, and which appeared as a combustible, exploding in its way from the chimney to the window. I clearly saw that no part came out from the fire-place.

Again begging you to excuse the roundabout way in which I have told my tale, I remain, dear Sir, yours faithfully,

JAS. LAYTON.

To W. H. WEEKES, Esq.

It is perhaps scarcely necessary to add that the house is not provided with any species of conductor; and, although it is surrounded by trees and ornamental shrubs, none of these rise to a height sufficient to occasion the deviation of a light-

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ning-flash from the adjacent building. The flash alluded to in the above letter of Mr. Layton, after striking the chimney-pot, seems to have divided; the principal current descending the funnel in a direct line to the earth, while another portion passed off laterally through the wall from whence it burst into the bed-chamber, and subsequently diffused itself in equilibrio with the adjacent walls, and other vicinal surfaces.

Sandwich, August 15th, 1845. W. H. W.

CXXV. *Phenomena attending some Thunder-Storms, at Coleford, Gloucestershire.* By J. Strickland, Esq.

The storm days were from the 4th to the 7th of September, 1844, inclusive. The previous summer was remarkable for drought, beyond any other within the memory of man. The first few days of September were more overpoweringly hot than any I had ever experienced; and that, too, in this hilly country, and within a couple of miles of salt water.

September 4th.—By 11 o'clock, the heat was such that I was compelled, for the first time in my life, to give in, on a shooting day, and seek for shelter.

September 5th.—Though fine overhead, the morning was ushered in by countless peels of loud but distant thunder. I went out partridge shooting before sun-rise; and observed dense masses of clouds towering in the distance, and approaching from the south, presenting unceasing discharges of lightning, and one unvaried roar of thunder. By 10 A.M., the storm had advanced even with me, but about 5 miles to the eastward; at which time, a second pile of clouds appeared in view, following a few miles behind the first, and in the same track; and filling the air with the unceasing bellowings of its gigantic artillery. Before noon, this second mass moved across in a northerly direction. By this time, a third mass, less replete with lightning, was receding slowly westward in the north-west horizon. I was thus in the centre of three storms. The air was now perfectly still, and no perceptible motion occurred in the clouds; although, from half hour to half hour, changes of place might be recognised. The sun was not yet obscured, although he was shorn of his brilliancy by the thickness of the air: it was oppressively hot, although I was on high ground, with steep cliffs and precipitous descents on three sides of me. I now began to experience electrical sensations; a soft, downy sensation filled my mouth and throat in breathing. Presently a rattling peal

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of thunder occurred over head; in an instant all was still again, when a beautiful, white flame descended around me to the distance of about 35 yards, and about 10 feet from the ground; it was perfectly level on its lower surface, with a mild, soft, velvety lustre, having none of the harshness of snowy whiteness, yet no perceptible colour; its motion was quick, but not rapid; it remained a very perceptible, I may say considerable, space of time. At this time the barrel of my gun appeared to reflect a dazzling light. A flash of lightning descended to this mass; it then spread horizontally to the sides of the field, shutting out from my view the sky and the upper half of the small trees for perhaps two seconds of time; it then suddenly opened in the middle and revealed the sky, when it receded along the tops of the hedges, disclosing tree after tree, and finally accumulated round a little tree, where it ended in a point. These successive phenomena were several times repeated, and in all cases it required some seconds of time for the tree to carry off the whole accumulation. The white light on my gun had at times so accumulated as to conceal the brown colour of the barrel, and to give the appearance of a white handkerchief having been thrown over it. I now left my gun in the middle of the field where I had been standing, and seated myself under a low part of the hedge, when I found, after being some 30 yards from the gun, that all the oppressive electrical sensations had departed.

As the thunder had now ceased, and all seemed over, I pursued my course; but had not crossed the next field before the sensations returned; the thunder cracked over-head, and the white flame occurred as before, accompanied with a heavyish fall of small rain. I left my gun, and took shelter, as before; this time my pointer appeared terrified, and lay perfectly motionless under my feet for more than a quarter of an hour; when I resumed my walk, and gained shelter in the woods from these influences.

My view of these phenomena is, that the earth, in its then dry state (for there had been scarcely a shower of rain since January), had become so much a non-conductor, that the electric fluid could scarcely obtain entrance except by roots of trees, or by projecting masses of solid rock; and that thus it lay piled, as it were, on the ground in masses or heaps.

I now emerged from the woods to a clearing of 100 acres in extent, in which small trees, 15 or 20 feet high, were left at 20 yards asunder; it was also studded with hazel-bushes, of a year's growth; it faced the north, with a gradual rise to the south, where it was bounded by the old wood. The reports of

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the thunder again increased in loudness, as the storm came on over the forest from the south. While reflecting on the awful roaring, and clanging, and bellowing that was gathering round me, I was startled by a tremendous report issuing from a huge oak about 100 yards behind me, which a flash now struck. Being alarmed for my safety, I placed my gun against a tree 20 yards in front of me, and sat on a stone to watch what was to follow.

I was soon startled by seeing a fire-ball pass before me within a few yards of the gun, and strike a small tree; others passed me, some having streams or tails of several yards in length, and all about 8 or 9 feet from the ground; they all ended by striking one or other of the little trees, within 200 yards of me; some passed over my head, others stopped short, and struck the trees in the wood; they all uttered a cauldron-like roar as they passed, and gave out a crackling din as they terminated their career. Dreading the close vicinity of these fire-balls, I lay at full length on the ground, watching and counting them as they flew; I observed that each spark spread to the size of the double fist, and emitted a dazzling light of a brilliant silvery whiteness: this scene lasted for more than a quarter of an hour; when the approach of a heavy fall of rain induced me to crawl to my gun, in order that I might protect it from the wet. As I raised myself for a moment to wrap my handkerchief round the locks, a violent cracking flash struck a little tree on the edge of the wood, 27 yards to my left. I instantly felt what resembled the heavy fall of a soft wool-sack on my head; the feeling passed down my chest, producing involuntarily a deep sob, and then down my stomach, producing a sensation of deadly sickness: my hands dropped from the handkerchief; I made two or three steps back to regain my seat, but fell to the ground; on recovering my recollection, which might be in a few seconds, I found myself choking and gasping for breath, with a most horrid smell of sulphur on my clothes. The rain now fell, much to my relief, for I was enabled to breathe again, and the sulphurous smell left me. The shower was violent in the extreme, and entirely darkened the air; but for the absence of weight and blow, I could have imagined myself under a falling stream. It was accompanied with large pieces of transparent ice, generally about an inch and a half long, and ¾ths of an inch wide, broken at the end. The lightning continued without diminution, often passing within a few yards of my head, as I lay on the ground a passive spectator of the conflict of the elements about me. As much by the noise, as by my recent shock, I was so overpowered as

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not to have energy to stretch forth my hand to take up one of the many pieces of ice that were upon me. Never less than two, and frequently as many as five of the balls of fire were in view at the same time; their flight was not so rapid as to be called instantaneous, for I could at times watch one through its course before another appeared; they all ended with two or more cracking reports. Having been exposed to this torrent for twenty minutes, I began to be anxious for shelter; and, by great exertion, I took my gun, and crept through the cover, but was checked by a report immediately in front of me; I saw no light, but felt an impression not unlike that produced by a swell, when bathing in the sea. I again placed my gun against a tree, from which, however, it soon fell, and I lay with my head and shoulders in one of the small hazel bushes. The storm now (2 o'clock P.M.) had attained its height, and I began to feel real alarm for my safety; the lightning poured in from every side,—the roaring fire-balls crossed each other in all directions, striking the trees at all heights, and also striking the ground. After a while, to my infinite relief, I perceived the lightning strike trees further down the wood; the confused mass of sparks thickened before me, till at last their number obscured all objects beyond them, when suddenly the rain ceased, the air became thin and delightful to breathe; the light increased, and the higher clouds became visible. Right glad I was to retreat from the wood, and regain the high road, and to reach my home. I felt ill with a distressing numbness down my right arm, side, and leg. I may here remark that my gun, though left to dry of itself, did not show the least appearance of rust; yet, a few weeks afterwards, having been wetted by a drizzling rain, with an east wind, the barrel soon became covered with rust, and careful cleaning has not restored it to its original brightness.

September 6th.—My eyes pained me this morning; in other respects I was better. I strolled with my gun to the hills which were the scene of yesterday's storm. The day was warm; a mist hung over the ground; a gentle breeze blew from the south-west. When I had arrived to about three-quarters of a mile from the wood, a most impetuous thunder-storm burst over it, advancing towards me. I gained a deep lane, and, having left my gun fifty yards distant, I leaned against the bank under a low hedge. The storm came rapidly on; the lightning descended in straight lines from high clouds, striking into the grass of the flat fields by which I was surrounded, and seeming to avoid the many trees that were near: it was not silvery white, like that of yesterday,

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but was of a deep red. The rain fell in torrents; the storm sped on rapidly, dropping its lightning every quarter and half minute. I imagine that, as the rain of yesterday had saturated the soil, and rendered it conducteous, the lightning of to-day found access to the substrata, without the assistance of the roots of the trees.*

CXXVI. *Electricity and Vegetation.* By A. Coventry, Esq. From "The Edinburgh Courant." Communicated by the Author.

(To the Editor of "The Courant.")

Moray Place, 14th July, 1845.

Sir—I have been making some experiments on electro-culture, but they are scarcely worthy of a place in a scientific journal, and I would hesitate to read them to a society; and yet, at this time, when the subject is exciting so much interest, it might be wrong to withhold them altogether from the public.

To Dr. Forster's experiments† at Findrassie we may trace all the present excitement; and it will give some idea of its extent when I mention that, besides a great number of trials now making in Scotland, in England there are 700 acres, and part of the Chiswick Gardens,‡ under experiment. But Dr. Forster's idea of applying electricity to vegetation was far from being a new one, although it was hailed by many as something unheard of. In truth there had been a very great deal written on the subject. Beginning with a person of the name of Maimbry, in this town, in the year 1746, it had engaged the attention of Jallabert, Sennebier, Bertholet, Du Hamel, Nairne, Van Marum, Muschenbrock, Carmois, Ingenhouz, Cavallo, De Candolle, Davy, Pouillet, Ellis, Becquérel, Noad, and a dozen more whom I could mention. The result to which it was generally understood that they had come was, that electricity of moderate force exercised no influence on vegetation, and that a strong charge was fatal. No doubt the Abbé Bertholet (who wrote in

* Note.—We have deemed it prudent to condense the above account, and in doing so have studiously avoided omitting any notable feature of the storms; we have regarded the spirit and, as much as possible, the language of the author.—Ed. *Elec. Mag.*

† Vide *Elec. Mag.*, Vol. II. p. 47.

‡ Vide Notice at the end of the present No. of *Elec. Mag.*—Ed.

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1783) had taken a different view, with the support of Nollet, Jallabert, Muschenbrock, and Du Hamel. But the opinions of Ingenhouz and Cavallo, and more lately of Pouillet and Becquérel, prevailed; and these were, that "the most impartial, diversified, and conclusive experiments have shown that electrization does neither promote nor retard vegetable life." I quote the words of Cavallo (vol. iii. p. 357); but the same sentiment is to be found in Becquérel, the highest of all authorities; and in his fourth volume, p. 185, he explains that the action of galvanism, if it be energetic, cannot but be injurious, since it must destroy the tissues and alter the natural constitution of the juices. He had formerly shown that whatever the influence of electricity may be on animals, the tissues of plants are unlike the nerves, and will not, like them, contract, and they are besides bad conductors of electricity, as a dead leaf shows. If, therefore, it acts in their case at all, it must be mechanically, by affecting the capillary action, or chemically, by changing the juices; and much change either way would appear to be hurtful.

Now that the controversy has been revived, I have, like many others, repeated Dr. Forster's experiment. I hesitated at first, it was so very improbable that if there really was any virtue in electricity, its many advocates, who had flourished in their turn for a century past, should all have failed to find it, and bring it into use. Still it was possible that the subject was not, after all, exhausted, which I find to be the opinion of Mr. Noad; and perhaps the public was unjustly consigning the use of wires to assist vegetation to oblivion, beside the worthless Paragrêles or poles employed at one time all over the south of France to keep the hail showers off the vineyards, when they deserved to rank rather with the lightning conductor and other useful things which we have seen triumph over controversy. Besides, none of the early experimenters had the benefit of our improved science, or our constant batteries.

Dr. Forster employed two tall poles, placed at some distance from each other, and standing due north and south, and along the top he had a thin iron wire stretched, which then descended a couple of inches into the earth, and made the circuit of the ground which he experimented on. The wires which I used were thin, and the poles were 16 feet high, the distance between them being 60 feet or thereabouts. The ground had been sown with barley, which was beginning to appear. In one point I deviated from the prescribed method. Owing to the ridges, I found it inconvenient to make the poles stand north and south of each other, and did

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not attempt it; and perhaps Dr. Forster will say, as Launce did to his dog, "Did I not bid thee still mark me, and do as I do?" There was no help for it. I did deviate; and, after all, the wires were not much defrauded of their full share of the current which some people suppose to sweep round the globe; for, to make up, I added twelve or fourteen sharp spikes, and soldered them to the wire, between the poles, which he omitted to do. And I may add, that several friends of mine adhering strictly to the rules came nevertheless to the same result with myself, which was, that the corn grew just as if there had been no erection of wires, and was quite undistinguishable from the rest of the field. Having alluded to these experiments, it may be right to state that one of them was made in Ayrshire, one near Bathgate, one in Perthshire, and two in Mid-Lothian.

It occurred to me next to try an alteration, and instead of leading the wires down into the earth, where the electricity would speedily be dissipated, perhaps without going into the roots, to lead it a few inches above the ground in a circle. The idea was taken from Abbé Bertholet's method, as shown in his first plate; and its design was to make the plants below the wire form the medium of communication between the air and the earth. It is well known that, generally speaking, the latter is negatively charged, and the former positively. But this experiment likewise failed, and the crop was neither better nor worse than its neighbours.

I now tried galvanism. After the method used by my friend Mr. Bain for his electric telegraph and clocks,* I sunk a quantity of coke, two feet deep and six feet wide, and (at some forty yards distance) the same quantity of zinc, and connected them by a copper wire, which was insulated with glass, and kept them from touching the ground. The weather, at first especially, was moist and favourable, but still I got no result of any value. This was disappointing—for the discoveries of Mr. Bain and Matteucci had shown that, if any benefit attended the use of galvanism, it was at least of easy application. Indeed a whole country side might be galvanized by running a long cordon of zinc at one end, and of coke at the other, when all between would be under the influence if these were connected by a wire.

Matteucci's very singular experiments were made with a galvanic battery of little power. He used, as I found on looking into the *Comptus Rendus* for 1844,† a single arrangement of Bunsen and rain water. Whether the earth would be

* Vide *Elec. Mag.* Vol. I. p. 606. † Ibid. p. 587.

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an equally good conductor of galvanism of high tension, as he had found it to be of low, is more, I believe, than is yet known; but, indeed, the results he arrived at were quite as unexpected and inexplicable as any yet to be tried can well be. But leaving this point in abeyance, and without pretending to determine anything in regard to the earth's conducting power for galvanism of high power, I thought at least of trying what effects the high power would have. It would, at all events, present the gases in a nascent state to the roots, with a variety of secondary actions. For this end, I took a battery of eight platinized silver plates, about 4½ inches square, and sixteen amalgamated zinc plates, and having charged it with sulphuric acid and water, in the proportion of one to nine, I inserted a plate of copper at the end of a small wooden trough, three feet, full of earth, and a zinc plate at the other, and connected the former with the zinc pole of the battery, and the latter with the silver. I then sowed a quantity of garden mustard, and in another trough had the same quantity sown in similar earth; and as to depth and watering, the two were treated alike. The plants in both appeared, but in no respect was there any difference that I could discover. They were allowed to grow to some height, but still without any perceptible difference.

This experiment I repeated afterwards with barley, and with the same result. To test the efficacy of electricity still further, I called in the aid of a second battery; and my last experiment was with barley grown under the influence of two of Smee's batteries, but I am sorry to say that I got nothing even then to encourage me to make any more trials. The last plants are now dried, and any one may see them.

If these experiments had been more successful, the subject would have been worth prosecuting; but I have no mind to go on. Perhaps I may resume it; but, lest it should be otherwise, I will mention what I thought of trying, in case there may be others who may wish to take up the inquiry. There is then an experiment mentioned in Brande's *Journal of Science*, vol. iii. (New Series,) p. 501; and in connection with it there should be considered a very curious experiment of the late Mr. Barry, which is noticed by Professor Faraday in his *Researches*, p. 95,* and in the article on *Electricity*, in the

* Two wires were sealed in connected tubes, containing sulphate of soda, coloured with syrup of violets; the wire of one tube was connected with the earth, and that of the other with the string of an insulated electrical kite; hydrogen appeared in the tube connected with the kite, and the liquid became green from the alkali evolved; oxygen appeared in the tube connected with the ground, and the liquid became red from the free acid. Faraday says:—343, "Mr. Barry's experiment is a very important one to repeat and verify. If con-

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Encyclopædia Britannica, p. 632. Again, the efficacy of electricity in raising water in capillary tubes may suggest certain experiments with plants; and I may refer to Porret's experiment, mentioned by Dutrochet, vol. i. p. 70, and by Becquérel, vol. iv. p. 200, begging to remind the reader, that it is pure water only which can be so raised, and not a mixture of water with acids. There is also an important point to settle between Becquérel and Sir Humphry Davy, the one maintaining that the negative pole favours germination, and the other that the positive; and both giving very plausible reasons, and very unhesitating statements. Lastly, Sir Humphry Davy has given us a method, as everybody knows, by which either an acid or an alkali may be injected into plants with the aid of galvanism; and it would be important to know how far this admits of being done, and with what effects on succulent shrubs, forest trees, and fruit trees, and on the fruits also when formed. Where all this may lead, no one can tell; but, looking to what the chemist has done in the last few years, we may well hold up our hands and ask, with Antonio, "What impossible matter will he make easy next?"—I have the honour to be, Sir, your very obedient servant,

A. Coventry.

CXXVII. *Description of the Apparatus shown at the London Institution.** Contrived and constructed by Mr. J. N. Hearder, Plymouth.

1. A large magnetometer, calculated for investigating the laws of electro-motive force, and the resistance of wires, together with the various conditions which modify the development of magnetism in iron, by the influence of electrical currents. It consists of a strong, and delicate steel-yard, supported between two stout iron pillars about 30 inches high, and which are firmly secured with a heavy base of wood about 4 feet long and 1 foot wide; the pillars are placed about 8 inches apart. Upon the short arm of the steel-yard is hung a steel hook, to which is attached a contrivance for raising or depressing a keeper, which is attached to its lower end, so as to adjust it to the height of magnets of various lengths.

firmed, it will be, as far as I am aware, the first recorded case of true electro-chemical decomposition of water by common electricity."—Ed. *Elec. Mag.*

* Vide *Elec. Mag.* Vol. II. p. 54.

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These magnets are secured by a contrivance upon the base board, which allows of their being brought exactly under the keeper. The long arm is accurately graduated, and has a shifting steel hook, to which may be attached weights varying from 1 drachm to 20 lbs.; it is about 3 feet long and is calculated to estimate 400 lbs., and will turn with $\frac{1}{20000}$ of the whole weight. A support is placed under the extremity of the long arm to keep it from falling too far on breaking contact. The magnets which have been used are of various kinds, and have been made as circumstances have required. One series consists of a number of bars of the best iron, 1 inch in diameter, and varying from 6 inches to 36 inches in length, made from the same bar well annealed in pairs, one of each pair being straight and the other bent into the form of the letter U. With these, the influence of length, as compared with diameter, has been ascertained. To these bars are fitted bobbins, which slide easily upon them, having known weights and lengths of copper-wire coiled upon them. With these, numerous experiments have been made, in order to ascertain the influence of the position of the coil upon the magnet, and the effects due to variations of distance from the poles, &c. Some interesting results have also been obtained by comparing the effects of the different conditions of length, thickness, resistance, &c., of the wires, in modifying magnetic development.*

Another magnet, which has been found by the author to be extremely convenient and useful, is as follows:—

A bar of soft iron, about 30 inches long and $1\frac{1}{2}$ thick is bent into the form U. Upon this is coiled a rope, formed of 24 strands of No. 16 copper wire, each 12 feet long. These strands were previously covered with cotton and thickly varnished with sealing wax, to insulate them from each other. They were then twisted together to form a rope, so that every strand might thus be brought into the same relative condition with regard to its position and influence upon the magnet. The ends of all these wires are left loose and numbered, so that they may be joined by their similar ends forming a conductor 12 feet long and 24 strands thick, or by dissimilar ends forming one continuous conductor 288 feet long and 1 strand thick; or they may be made into a great variety of forms, varying in length and thickness. Batteries may be also attached to the separate wires, and the effects of opposite or coincident currents may be ascertained under a great

* The *Cornwall Polytechnic Society* awarded Mr. Hearder their first silver medal for this magnetometer.—*Ed. Elec. Mag.*

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variety of circumstances. Some of the results which have been obtained in the course of investigation with this instrument, will be given in a future number.

2. A powerful electro-magnetic engine:—It consists of two circles of magnets placed horizontally, the one over the other, upon a stand. The lower one is fixed, and the upper one revolves by the inversion of the polarity of its magnets. Its construction is as follows:—A stout ring, 16 inches in diameter, is formed of iron and is fastened upon the four extremities of a strong cross, through the centre of which an axle passes and is secured to and revolves with it. Into the ring are riveted bars of iron, not projecting as radii from its edge, but inserted into one of its sides, and standing parallel with the axis. These bars are 24 in number; they are 3½ inches long, 1 inch thick, and are placed 1 inch apart. Around these are coiled alternately, right and left, 300 feet of No. 10 wire, the two ends of which are fastened to two stout copper rings placed within, and in connection with, the ring of magnets; from these rings copper bars proceed to the commutator placed upon the axle.

The arrangement of the wire upon the magnets produces in them alternately north and south polarity, so that they form in fact a series of 12 horse-shoe magnets, the ring serving as a common band for the whole, no magnetism being discoverable in the ring when the engine is magnetized. There is also an arrangement for uniting the wires so as to use them in 1, 2, 3, 4, 6, or 12 lengths, in order to ascertain what is gained or lost by varying the battery and resistance of the wire. Another ring, similarly constructed, but without an axle, is secured firmly upon the base exactly under the first, which is placed with its axle standing vertically in the centre of the lower ring. By this arrangement it will be seen that the magnets in the two rings will have their poles exactly opposite, and as near as necessary to each other. The distance between the magnets being barely equal to their thickness, it will be seen that, before any magnet in the upper ring is out of the influence of the opposed one in the lower ring, it is under the influence of the next. The polarity of the lower ring is not changed by the commutator, but merely that of the upper one. When the two rings are in such a position that dissimilar poles would be exactly over each other, inversion takes place and attraction is set up between each bar of one ring and the next of the other, and by this means a powerful rotation is set up. The revolving ring which, with its wire, weighs about 60 lbs., makes 300 rotations in a minute, with 12 pair of Smee's small batteries;

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and drives a lathe with considerable force. The power is sufficient to raise 40 lbs. a foot per minute with 12 pair of plates 4 inches by 7. With a larger battery and more extensive series, greater effects might be produced, but the author has not yet had time to prosecute any experiments with it. By a second commutator attached to the battery, the rotation of the engine may be stopped or reversed in a few seconds.

3. A bar of iron and delicate steel-yard, mounted on a stand, for ascertaining the effect produced by varying the distance of the coil from the surface of the magnet. This instrument is calculated for delicate experiments.

4. An *electro-statiscope*, for ascertaining the temporary electrical condition excited in bodies vicinal to electrified surfaces during electrical discharges. This apparatus was called into existence during some investigations on the so-called lateral explosion. The fact that the spark, which had been obtained by Faraday and others from wires that were employed to conduct away sparks from an electrical machine, would not affect a delicate gold-leaf electrometer, induced a belief that the spark was not *given out* by the wire, but *taken from it* by the effect of the inductive action of the electrified body upon adjacent surfaces.

The apparatus consists of a small Leyden jar, supported horizontally upon a glass pillar, which turns on its axis. Two wires, pointed at one end, slide horizontally through caps placed upon two other glass pillars, which are arranged one on each side of the centre pillar, and in a line with it. The Leyden jar being adjusted in the same line, the two sliding wires are brought with their points very near to the knob and bottom of the jar. If the instrument be placed upon a table, and a wire be connected with one of the insulated wires at one end, whilst the other end rests upon the ground at a short distance from an electrical machine, and a similar wire be connected with the other insulated wire of the apparatus at one end, whilst the other end also rests upon the floor, but at a greater distance from the machine, every time a spark passes from the conductor to a wire carried out of the room or between the conductor and the rubber of the machine, a spark passes between the knob and bottom of the jar and the insulated wires of the apparatus, and the jar receives a slight charge, showing the existence of a temporary disturbance in the electrical state of the apartment, and the direction of the current thus set up to restore the normal condition.

A future number will contain the result of the applicaion of this instrument as a test for ascertaining the elecrical con-

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dition of bodies during the passage of electricity through or near them.*

5. A condensing electrometer, applicable to the same purpose as the preceding instrument; by means of them plus and minus sparks can be obtained from the same part of the same wire, and at the same instant of time.

6. A new battery-discharger, consisting of an insulated ball to be connected with the inside of the battery; a second glass pillar is placed about six inches distant from the first, having on the top of it a knob, in which works one end of a brass lever, to the other end of which is attached a knob, the lever being long enough to allow of its resting upon the beforementioned insulated ball at the time of the discharge.

When the instrument is set, the lever with its knob is lifted up and turned back beyond the vertical line, and would fall back in the opposite direction were it not for a short tail-piece on the other end of the lever, which supports it nearly vertical, but so far back as to be free from any liability of falling over upon the positive ball. A second insulated lever is placed so as to act upon the short tail of the discharging lever; and, by pressing upon this, the latter is thrown forward and falls with its knob like a hammer upon the ball connected with the battery, and the discharge is effected.

NOTE.—The apparatus numbered 4, 5, and 6, were exhibited at the Royal Institution on Friday, May 23rd; and at Mr. Gassiot's soirée, June 21st.

7. *A Cast-Iron Magnet.*—It being a desideratum to employ permanent magnetic power to as great an extent as possible in electro-magnetic engines, and it being also desirable to get this power at as cheap a rate as possible, it was thought that cast-iron, although its magnetic capabilities were limited, when used in extensive series, might be made available for this purpose. Twenty-four horse-shoes were accordingly cast of the best pig-iron, as hard as green sand would make them. Their weight was about 3 lbs. each, and each was magnetised in the ordinary way; and when united, the magnet, weighing about 70 lbs., is capable of lifting 60 lbs., and the power is extremely permanent. The bars have been put together promiscuously, the strongest not having been selected, and although it is known that there are several of the bars almost neutral, yet the power is such as we have just stated. Such a magnet as this can be made for about 12s. or 14s., whereas a steel magnet of equal power would cost two or three pounds.

* Mr. Hearder has presented this instrument to Mr. Bachhoffen, of the Royal Polytechnic Institution.—ED. *Elec. Mag.*

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CXXVIII. *Galvanic Music.* By Prof. Page.

Washington, D. C., June 2nd, 1845.

Sir,—I notice by the two last numbers of your interesting Journal (Nos. 7 and 8), that the production of musical sounds by magnetism has excited considerable attention; and in Vol. I., No. 7, I perceive you have dated the observation of this fact to Mr. Joule, March 1842. Permit me to call your attention to my reclamation upon this subject, as published in the April number of *Silliman's Journal* for 1845, by which you will perceive that I published in the same journal, in 1837, an account of this curious phenomenon, under the title of *Galvanic Music;* and another notice of the same in 1838, *Silliman's Journal,* Vol. xxxiii. page 118, under the title of the *Disturbance of Molecular Forces by Magnetism.* Shortly after the latter publication, I performed some more striking experiments, by which the sounds were prolonged, the experiments being based upon the observation made and noticed in the last-named announcement of the fact. Steel bars were carefully suspended within a series of coils, and the galvanic circuit in these coils was broken at the rate of five or six thousand times per minute, by a revolving apparatus placed in an adjoining room, for the purpose of quiet, where the sound was to be heard. The oft-repeated disturbances of the bars caused a prolonged, smooth tone; and by connecting the separate coils in series with the revolving apparatus, the various notes in the scale were readily produced. I have often thought that a good musical effect might be produced in this way. As I had the pleasure of frequent interviews with Mr. Lettsom, in Washington, I presume he intended, in his communication with Mr. Weekes,* to have named me as the first observer of the fact.—I am, very respectfully, your very obedient servant,

Chas. G. Page.

Charles V. Walker, Ed. *Elec. Mag.* London.

SCIENTIFIC MEETINGS.

BRITISH ASSOCIATION, 1845.

The Association met this year, at Cambridge, from June 18th to 25th; upwards of one thousand members attended, and many distinguished foreign savants.

* Vide *Elec. Mag.* Vol. I. p. 528.

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Wednesday, June 18.—*General Recommendation.*

That it be recommended to the Council to consider whether the *Electrical Experiments at the Kew Observatory* should not be discontinued; some debate having arisen on this point, it was stated that the discontinuance had no reference to pecuniary considerations, but arose from the fact that similar observations were now being made at the Observatory at Greenwich, under the superintendence of Professor Airy.

Recommendations from the Magnetic Conference.—Presented by Colonel Sabine.

That it is desirable to have the magnetical and meteorological observations at Greenwich and Trinity College, Dublin, continued. That those at Toronto, Van Diemen's Land, and St. Helena, be continued to December 31st, 1848. That the apparatus at the Cape of Good Hope be transferred to the Astronomical Observatory. That it be represented to the East India Company that the observations at Simla and Singapore may be discontinued, but that it is desirable to maintain those at Madras and Bombay. That it is desirable to continue Captain Elliot's magnetic survey of the Indian Seas. That it is desirable to continue the magnetic survey of Canada, and to connect Toronto with the observatories in the United States. That Colonel Sabine's establishment at Woolwich should be maintained, for the purpose of reducing observations and preparing them for publication. That the co-operation of foreign governments having been found highly advantageous, her Majesty's ministers should be requested to express this opinion to the governments of other countries. That the members of the present Magnetic Committee be requested to continue their services; and that the names of the Marquis of Northampton, Sir J. Lubbock, Professor Christie, and Professor J. D. Forbes, be added to the list.

Resolved, in conformity with the expressed opinion of the Magnetic Conference, sanctioned by the Committee of Recommendations,—"That it is highly desirable to encourage by specific pecuniary reward, the improvement of Self-recording Magnetical and Meteorological Apparatus; and that the President of the British Association and the President of the Royal Society be requested to solicit the favourable consideration of her Majesty's government."

Thursday, June 19.—Mathematical and Physical Science.—Mr. Dent described his *Mode of suspending a Ship's Compass.** Mr. Dent, in reply to Dr. Lloyd, was aware that Mr. Fox's dipping needles were similarly suspended; for he had himself constructed the instruments: but the present was the first attempt to suspend the ordinary azimuth compass in a similar manner.

Dr. Scoresby described a *Large Magneto-Electric Machine;* the magnets of which were built up of small magnetised bars, on principles with which our readers have already been made acquainted.† A magnet of this kind, of the size of a lower mast of a first-rate ship of war, would produce a deviation of nearly 1′, at the distance of a mile. The effects of Dr. Scoresby's machine, with a very imperfect armature, were,—it decomposed water, at the rate of a cubic inch of the gases per minute; copper was reduced from its sulphate, at the rate of 1·2 gr. per minute; sparks were visible in daylight, when the armature made but one revolution in fifteen seconds. Professor Forbes admitted the power of this arrangement; but

* Vide *Elec. Mag.* Vol. II. p. 76. † Ib. Vol. I. pp. 222-7.

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thought that electro-magnets were preferable, as they were so easily obtained.

CHEMISTRY AND MINERALOGY.—*On the Voltaic Reduction of Alloys.* By C. V. WALKER.—The object of this communication was to show the means which had been adopted by the author for obtaining an insight into the nature of the electrolysis, by which an alloy is reduced.* A cathode of mercury was substituted for the ordinary metal, the result was the formation of amalgam of potassium; but neither zinc nor copper were reduced from the solution. The inference, therefore, was, that the cyanide of potassium is decomposed by direct electrolysis; and that the potassium takes the place of the metals, zinc and copper; which latter were therefore, reduced by secondary action.

Friday, June 20.—MATHEMATICAL AND PHYSICAL SCIENCE.—*On the Influence of Friction on Thermo-Electric Phenomena.* By Professor P. ERMAN.—The author wished to find out the mean course of truth between the two extremes of electric omnipotence and electric nullity, which have been attributed to the friction of conductors. He examined the influence of the friction of two heterogeneous metals, by Nobili's multiplier. The first step to which his experiments led him was, that the effect of such friction is always like the addition of heat to the point of contact. One method of showing this was by the fact that groups of metals, which give rise to an anomalous electric current, by difference of temperature, act in a similar manner when rubbed. This applies to galena, sulphuret of molybdena, and some others. In allusion to this intermediate office of heat, he calls such dynamical effects *tribo-thermic electricity;* but, when exerting this its tribo-thermic effect, it is neither like conducted nor like radiant heat: its production, when friction begins, and its disappearance, when it ceases, are independent of the *mass* of the rubbing bodies, and almost independent of the duration of the process. He thinks that these facts are highly favourable to a peculiar kind of molecular vibration, excited exclusively in the rubbed points, and spreading through the conducting medium as easily as does electricity. Connected with this fact, is a circumstance which M. Erman thinks will be used for the measurement of tribo-thermic effects almost in the same way as the two fixed points of our thermometer are for temperature, viz.,—there exists, for any group of thermo-electric metals, a given positive and a given negative difference between their temperature and that of the surrounding space, which, when previously existing at the point of contact, does not, by continued friction, exercise any influence on the electric current. The author reports some facts in connection with Peltier's discovery of the production of cold by a current passing in one direction between two heterogenous metals. He examined the effect of friction on bismuth and antimony in the four following cases:—

* Vide *Elec. Mag.* Vol. I. p. 559.

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When heated.	When at the temperature of surrounding space.	When cooled.
Antimony.	Bismuth.	
Bismuth.	Antimony.	
	Bismuth.	Antimony.
	Antimony.	Bismuth.

He found that, in these different cases, the same act of friction produces at one time a gain of electricity with a loss of heat,—at another time a loss of electricity with a gain of heat; again, either a loss or gain of electricity, joined to a loss of heat: and he asks, whether heat when nascent has not a property specifically different from that of heat residing in a metal? whether we are not on the eve of finding something analogous to Peltier's discovery? M. Erman's chief object in this communication was to induce British philosophers to engage in tribo-thermic electrical investigations. In reference to the imperfect character of these investigations, he alluded to the unavoidable influence of heterogeneous rheophores; which, however, he had partly obviated, by interposing a plate of pure plumbago between each metal and its conducting wire. In conclusion, he suggests a practical application of this tribo-thermic electricity. Instead of a voltaic apparatus for the electric telegraph, he proposes this purely mechanical mode of exciting electricity, so as to affect a distant magnetic needle, and thus transmit signals. It is accomplished by simply moving a detent from a piece of clock-work, when a disc of bismuth rubbing against one of antimony, excites the distant needle. He has tested the method for tolerably long distances.

Supplementary Note.—M. Erman communicated to the *Athenæum* (August 30), a note of which the following is a translation:—

"An eminently qualified judge (Mr. Grove) having found that I have given very imperfect grounds for my suspicion of a possible analogy between certain effects of nascent heat in the state of friction and M. Peltier's discovery, I condemn myself for having adopted a too strictly aphoristic form, and I endeavour to compensate for it, by choosing, from among several, a tribo-thermo-electric fact, the extreme paradox of which has especially led me to entertain the suspicion.

"Let there be a *crystal* of sulphuret of lead at one of the poles of a multiplier, and at the opposite pole (perhaps alternately put into play) the rheophores of a bar of bismuth and a bar of antimony. The bismuth, rubbing the crystal, immediately acquires negative electricity. This exception was already known for heated bismuth. From all analogies at present known, it follows that antimony rubbing in its turn will become positive, and that, to obtain from it a negative declination, it would be necessary to cool it. Now I find that the rubbing of the antimony gives absolutely the same declination as the bismuth; direction, intensity, promptitude,—all is sensibly equal. It cannot be denied that, in this excessively paradoxical case, it seems that an increment of nascent heat produces, in respect to antimony, the effect of a cooling.

"The singular effects that are observed, on substituting lumps of sulphuret of lead that are not regularly crystallized, confirm the view that the effects of rubbing are subordinate to molecular movements. I hope that more skilful phi-

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losophers may succeed in obtaining tribo-thermic effects by the simple internal vibration of sonorous elastic rods. I have not yet been able to obtain them. But the grand prize will be his who shall discover a difference of thermo-electric reaction, according as a body endowed with magnetic polarity is rubbed (molecularly heated) at one or other of its poles.

"Loadstone (*pièce d'aiman*, Fr., *magnet-stein*, Ger.), and magnetic sulphuret (*magnet-keis*, Ger.), react very powerfully by rubbing; I have employed them, but hitherto without success, as well as magnetized steel, for this singular investigation, which may very ridiculously be named, *magneto-tribo-thermo-electric*."

On the Baron de Bode's Insulated Compass. By J. Y. Oliver.—The object was to insulate the compass from the iron of the ship. The contrivance was this: a double glass bowl, the intermediate space being filled with mercury, was made to act as the bowl of the ordinary compass. It was hung in gymbals, and protected with lead, which rendered it very heavy and cumbersome. Mr. Dent objected, that, if the insulation would protect the needle from the action of the ship's iron, it would also shield it from the directive force of the earth, and therefore render it useless; but on placing a poker near the compass, it was distinctly affected through the mercury.

This Report will be concluded in our next number. The other papers are,—Sir John Herschel's Report; Mr. J. A. Brown, on Sir Thomas Brisbane's Observations; Mr. Saxby, on Magnetism as connected with the Earth's Structure; the Gas Voltaic Battery, by Professor Grove; Professor Schöenbein, on Ozone; Magnetic Disturbance, Rev. H. Lloyd; Laws of Static Electricity, W. Thompson; Journal of the Kew Observations, F. Ronalds, Esq.; the Chemical Equivalent of Heat, J. P. Joule; Ozone in the Atmosphere, Professor Schöenbein; Hydro-Electric Machine, W. G. Armstrong; Endosmose and Exosmose influenced by Galvanism, Dr. Leeson; Aurora Borealis, Rev. J. Fisher; Influence of Galvanic Electricity on Germination of Seeds, Professor E. Solly; Electrotyped Plants, Capt. Ibbetson.

ROYAL ACADEMY OF SCIENCES, PARIS, 1845.

(*Continued from p.* 60.)

June 2.—*Extraction of Copper from its Ores by Electric Action.* By MM. Dechaud and Gaultier de Claubry. (Vide *Elec. Mag.*, Vol. II. p. 103.)

June 9.—*On Magneto-Electric and Electro-Magnetic Facts.* By M. Arago.

In presenting to the Academy M. de Haldat's *Histoire du Magnetisme, dont les Phénomènes sont rendus sensibles par le mouvement*, M. Arago corrects an error of the author, in attributing to Messrs. Herschel and Babbage the discovery, "that rotating metal discs lose a large portion of their power when solutions of continuity are made in the direction of their radii;" whereas, in the memoir published by these philosophers, in vol. cxv. of the

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Philosophical Transactions, p. 480, they say that their experiments were made "*after* M. Arago."

M. Arago alludes to another inaccuracy, which had occurred to "one of the most illustrious men of our epoch:"—

"In his beautiful work, Mr. Faraday, whose friendship," says M. Arago, "is so dear to me, attributes to M. Ampère the discovery of the motion which a wire, traversed by a voltaic current, acquires, when it is placed horizontally at a certain distance from a rotating metal disc. The following are the facts:—

"About the beginning of the month of August, 1826, M. Arago thought that his experiments of rotation might be attempted, by substituting currents for magnetic needles. Not having a pile, he begged his friend, M. Ampère to have the apparatus set up in the museum of the College of France. The assistant, M. Ajasson de Grandsagne, made the necessary arrangements; but, on the day when the first trial was made, at the very moment when the wire began to move, the rotating axis of the plate broke. M. Arago, setting out the next day for the Pyrenees, authorized M. Ampère to continue the experiment. M. Colladon presided at the reconstruction of the instrument, and introduced into it important improvements. This time the wire moved almost at the very instant, when the copper plate began to turn. M. Ampère hastened to transmit the result to his absent friend.

"These explanations scarcely seem necessary; for, on publishing the experiments, M. Ampère quoted M. Arago. But since the note of the illustrious and so-much-to-be-regretted philosopher has deceived such a man as Mr. Faraday, it has not been deemed useless to place M. Ampère's letter before the Academy. M. Arago found it this week by chance: we extract merely the following passage:—

"'*Paris*, 1*st December*, 1826.

"'You will see in this note that I have been careful to say that the idea of this experiment belongs exclusively to yourself.

"'It remains for me, my dear and very excellent friend, to remind you that you promised me, if this experiment should succeed, to defend my theory as the true explanation of the phenomena. In joining it to all the rest, and to the calculations of the memoir, which is printed among those of the Academy, I do not see what further can be objected to me.

"'I have also to beg you, if you find the note, that I send you, as it ought to be, to write to M. Savary to insert it as it is in the *Annales de Chimie et de Physique*, except all the changes and additions, which you are at perfect liberty to make in it, since the experiment was devised by yourself.'

"We may add,—for this has never been printed,—that, immediately after his return to Paris, M. Arago repeated with currents the experiments already made with needles, and that they gave the same results with regard to the direction of the forces when

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using entire discs, and with regard to their weakening when using cut discs."

On the Electric Telegraph between Paris and Versailles. By Mr. WHEATSTONE.

The magneto-electric telegraph which was submitted to several members of the French Academy, at the college of France, in 1841, has been in action since the commencement of the present year, at the first two stations of the Paris and Orleans line, and on the Paris and Versailles line. There is now a daily communication beween Paris, St. Cloud, and Versailles. The instruments are:—an alarm to call attention,—an electro-magnetic telegraph, worked by the magneto-electric machine, by which words can be spelled and signals sent at the rate of twenty-five per minute,—a similar electro-magnetic telegraph, with arrangements for printing the signals, as they are presented, either in letters or figures. Other instruments are to be added. The instruments now set up have existed in England since 1807, and have had their last form since 1840. They have been put to the severest tests, and have always triumphed. The signals have traversed a distance of 352 miles; and, whether excited by the pile, or by the magneto-electric machine, they have not failed. Before leaving Paris, Mr. Wheatstone invited such members of the Academy, as had not yet seen the apparatus in action, to witness it on the Versailles line.

On the Electric Telegraph between Paris and Rouen. By M. ARAGO.

After the above note was read, "M. Arago announced that the electric telegraph established between Paris and Rouen, under the inspection of a committee, including four members of the Academy, acts in the most satisfactory manner. During the stay of the king at the chateau of Bizy, the electric communications with Paris occurred day and night with perfect regularity.

"M. Arago gave an account of certain perturbations, easy to be explained, which occurred in the march of the apparatus during storms."

On the Action of Magnetism on all Bodies. By M. ED. BECQUÉREL. (Vide *Elec. Mag.*, Vol. II. p. 106.)

At the request of the authors, MM. GAULTIER DE CLAUBRY and DECHAUD, the Academy appoints a committee consisting of MM. Becquérel, Berthier, and Dumas, to examine their *Process for extracting Copper from its Ores by means of electric actions.**

On the Electro-Magnetic-Chronoscope. By M. BREGUET, in reply to Mr. Wheatstone.

M. Breguet states, that his only part "in the construction of the machine consisted in the application of the physical and mechanical means, dependent on the art which he cultivates; that he acted under the instructions of M. Konstantinoff, who acquainted him that he had spoken of this problem with Mr. Wheatstone, and which cir-

* Vide *Elec. Mag.* Vol. II., p. 103.

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cumstance he had not omitted to mention. He believes that his is the better process; and says that he is waiting for the results of the experiments from St. Petersburg. He mentions that he showed Mr. Wheatstone the drawing of M. Konstantinoff's machine, in December, 1844, and that it was not objected to. But it does not appear that he mentioned his intention of *publishing* it.

M. CHUARD gives the description of a new system of *Electric Telegraph.*

June 23.—*M. Palmieri* writes to M. Arago to state that the experiment claimed by M. Linari, and described at the meeting of March 24 (*Elec. Mag.* Vol. II. p. 57), had been previously communicated by himself, in the month of September, 1844, to the Academy of Sciences at Naples. His memoir is published in No. 17 of the *Rendi Conto* of that Academy; and describes the cases in which the extra current increases the spark and shock; or increases one at the expense of the other; or banishes both. He says that he made use of spirals both with and without iron wires.

M. Palmieri alludes to another note of M. Linari's (which we did not notice, as its contents were not given, but merely a few words as to their nature), in which M. Melloni is accused of having given a portion of the credit of one of M. Linari's own experiments to M. Palmieri. He knows not what is alluded to, and hopes M. Melloni will notice it; which he does in the following—

Remarks on M. Linari's Reclamation.—Letter to M. ARAGO from M. MELLONI.

M. Melloni cannot tell precisely the experiments to which M. Linari alludes; but presumes that one or other of two reports made by him to the Academy at Naples must be referred to. These reports concerned *the decomposition of water, the shock and the electric spark obtained by means of currents of terrestrial induction;* but since these were given as the *joint* discoveries of the two philosophers,* the only alternative was to divide the honour equally. M. Melloni has carefully re-read his memoirs, and cannot find a single word to justify this strange reclamation. Other experiments of the two philosophers have given rise to other groundless reclamations; so that M. Melloni prepared a history of currents of terrestrial induction to be read before the Naples Academy on June 3, 1845. The translation was forwarded to the French Academy; but being too long for insertion in the *Comptes Rendus*, it appears in the *Annales de Chèmie et de Physique.*

Remarks on M. Pouillet's Memoir on Apparatus for Measuring the Velocity of Projectiles.† In a Letter from Prof. JACOBI to M. ARAGO.

M. Jacobi mentions that a letter of his was presented to the Imperial Academy on January 31, 1838, describing experiments made at Dorpat, in 1836 or 1837, in which he had employed means nearly similar to those lately described by M. Pouillet, for obtaining a current whose duration was $\frac{1}{9000}$th of a second. He

* Vide *Elec. Mag.* Vol. I. pp. 80 and 335. † Ibid. p. 565.

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mentions that the result of his experiments gave not less than 1,260,000 ft. per second as the velocity of voltaic electricity. He also adds:—

"For about two years, I have employed for electro-telegraphic experiments an apparatus of a particular construction, which I call an Acoustic-Telegraph, on account of the continuous sound which is produced by continuous currents that are interrupted 150 or 200 times per second. As this telegraph transmits the sound to a distance of 25 k. m. (15½ miles), or through a circuit of nearly 50 k. m. (31 miles), we may conclude, conformably to the views generally adopted, that the velocity of electricity is not less than 7,500 or 10,000 k. m. per second, or, if you please, 20,000 k. m., providing that the current be formed and disappear 200 times per second, as I announced in a public discourse held at the commencement of last year, and printed in the *Recueil des Actes de l'Académie*. This latter mode of experiment may at the same time serve to decide the following question:—Does the limit of velocity depend upon the absolute length of the conductor, or only on its resistance?"

M. Jacobi mentions that he does not in this allude to M. Pouillet's application of the galvanometer as a ballastic pendulum. And he cannot help remarking that this proposition "cannot advantageously contend with the ingenious electro-ballistic apparatus of our talented artillery officer, M. Konstantinoff." This latter expression induced M. Arago to remark that the question of priority,* in respect to the apparatus for measuring the velocity of projectiles, involves M. de Konstantinoff and not M. Breguet.

Extract of a letter from M. WARTMANN, *concerning Researches on Electric Interferences.*

"I seize this opportunity to beg you to communicate to the Academy the following result, which cannot fail of being of some importance in the theory of electricity. I have been seeking directly whether this imponderable agent has the property of presenting phenomena of interference, analogous to those of sound, light, and caloric. With this view, I employed three distinct methods; that of *induced currents* in a wire placed symmetrically between two similar wires, through which were sent at the same time unequal currents in the same or in a contrary direction;—that of *direct and continuous currents*, compelled to traverse simultaneously the same wire, and which were made to vary by degrees, infinitely small, from equality to extreme inequality;—finally, that of *deviations* exercised in three different ways upon the same constant current. They all led me to recognise that *no interferences are produced, even under the most favourable circumstances.* I repeated some of my experiments before the Society of Natural Sciences at Lausanne, assembled on the 9th of March last; and I also communicated to the Society of Physics at

* Vide *Elec. Mag.* Vol. II. p. 86.

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Geneva, in its sitting on April 17th, my memoir, which will very soon appear in the *Archives de l'Electricité.*

M. Delezenne describes an *Electric Hoop;* but as his note was not sufficiently clear, he was requested to furnish further particulars.

ROYAL ACADEMY OF SCIENCES AT BRUSSELS.

(*Continued from p.* 63.)

Feb. 1, 1845.—*Researches on the Causes of Barometric Variations.* By M. Peltier.—(Report by MM. Crahay, d'Omalius d'Halloy, and Quetelet, reporter.)—The author commences by calling attention to the existence of four sorts of variation in atmospheric pressure: 1st, Those which appear to be due to accidental causes, and which are without apparent regularity; 2nd, Those which vary according to the hour of the day; 3rd, According to the latitude; and 4th, According to the proximity of the tropical current, namely, the current of heated air, which sets out from the equator, and is directed toward the poles; variations that may be distinguished by the names of *accidental*, *hourly*, *latitudinal*, and *altitudinal;* he then goes on plainly to set forth the facts, in order to explain them. We will here speak only of the facts relating to the last two species of oscillations, which have engaged less attention.

We know that the mean pressure of the air, when reduced to the level of the sea, varies with the latitude;* it increases from the equator to the thirtieth degree, and then diminishes to the sixty-fourth, in order to increase again to the poles. The pressure along a meridian, therefore, has two *minima* and two *maxima;* moreover, the isobarometric lines are not parallel to the equator. Thus Nice and Avignon, situated under the forty-fourth degree, have, the former, a mean pressure of 759·087^{mm}, (29.875 in.), and the latter, a mean pressure of 760·824^{mm} (29·944 in.)

With regard to altitudinal oscillations, we know that, on rising in the atmosphere, the pressure diminishes, gradually but unequally, according to the localities. The *minimum* of the morning first disappears, then that of the evening. On approaching the region of perpetual snows, the evening *minimum* entirely disappears, whilst a new morning *minimum* appears, which is more extensive than that of which vestiges may remain in the neighbouring plains; and it appears again two hours later.

When we have reached, and we pass the limit of perpetual snow, there then remains but one *maximum* about 9 P. M., and one *minimum* between 6 and 7 A. M. In high latitudes very nearly the same hourly variation is found as on snowy summits; a *maximum* about 8 P. M. is distinguished, and a *minimum*, prolonged from 7 A. M. to noon.

The object is to explain these very various phenomena; at the

* Vide *Kaemtz's Meteorology*, chap. v. passim. London, 1845.

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same time the numerous anomalies that are observed in measuring heights, by means of barometers, must be accounted for. These heights, according to the most practised observers, sometimes present very striking discordances for the same stations, according to the hour of the day when the observation is made, according to the state of the sky, as well as the electric tension and sign of the vapours.

Without entering into all the details given by M. Peltier in support of his theory, we shall produce verbatim the propositions in which they are summed up :—

"The general pressure of the atmosphere is the result of two simultaneous pressures of the air and of the vapour. The air, being a permanent gas, and a non-conductor of electricity, does not experience any direct alteration, except in its density, by variations of temperature. Vapour, on the contrary, besides variations in its density, changes its state ; it is, moreover, a conductor of electricity, in different degrees, according to its density. The perpetual changes of the vapour incessantly alter its pressure; its dissemination in the medium of air, by communicating to the latter its electric mutations, its attractions and repulsions, it makes it participate in all its perturbations, and thus considerably increases the results.

"As the globe possesses a resinous tension of its own, the vapour that rises from it is resinous likewise.* An elastic fluid is formed by the union of attenuated particles, at great distances and independent of each other; when these particles are electrized, they all repel each other like light bodies, and they have then a greater volume. Then every electrized fluid is less dense than a neutral fluid. When all the particles are at uniform distances, the interior electric reactions are equal in all directions, it is only the particles on the circumference that experience a less reaction on their exterior segment; it is in consequence of the reduction in the reaction which these segments experience, that electric disturbances are directed to the surface. When a mass of electrized vapours is in the neighbourhood of a body charged with the same electricity, all its molecules are individually repelled; moreover, the peripheric portion of the electricity of the cloud, which is on the sides of the body, is repelled toward the interior and increases that of the particles; this double action produces two distinct effects, the entire mass recedes, the molecular repulsion, and consequently the total volume, is increased. When the neighbouring body is charged with a contrary electricity, it attracts the individual particles, and, by its attraction it increases the peripheric discharge; the cloud, as a mass, approaches; the interior repulsion, as well as the total volume, diminish. In the first case, the vapour weighs less for equal volumes; in the second, it weighs more.

"As vapours possess a semi-conduction for electricity, the resinous influence of the globe repels the electricity of the same name towards the elevated regions, and gives the inferior regions a contrary or vitreous state. From this distribution of electricity, it follows that the specific weight of the upper vapours is diminished, and that of the lower vapours is increased. The air, by its intermixture with the vapour, participates in its resinous and its vitreous tension, and thus increases the result, which belongs to the vapour. The pressure will, therefore, increase according to the thickness of the inferior vitreous stratum, according to the energy of its tension, or according to the relation of the contrary quantities and tensions of the inferior vitreous and the superior resinous strata. A slight fall of temperature is sufficient to increase the conduction of the vapours, and to change the distribution of the electricity, and consequently the atmospheric pressure. In like manner, an elevation in the temperature re-vaporizes the opaque vapours, other vapours rise from the ground; the quantity of resinous vapours increases, and the pressure diminishes.

"The globe, being a body charged with resinous electricity, receives with difficulty that of the same name, which is possessed by the vapors; these lat-

* Vide *Elec. Mag.* Vol. I. p. 412.—Ed.

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ter cannot be neutralised, except by the vitreous vapours, which it attracts, or when their density overcomes the repulsive power of the globe; they then fall, they discharge themselves on the ground, which is less resinous than they are themselves, by brisk exchanges and by squalls of wind. The vapours and the dilated air, which constantly rise between the tropics, form a current toward either pole, charged with the same resinous electricity as the globe, whence they are derived; this current encloses the mass of diurnal vapours, between two forces of the same nature, acting in opposite directions. Under this double resinous influence, the intermediate vapours form three distinct strata, in regard to their electric tensions: the vapours or clouds of the upper stratum are vitreous, on account of the resinous or negative influence that is above them; the inferior stratum is in like manner vitreous from the influence of the globe; finally, the clouds of the intermediate stratum are the receptacle of the negative electricity repelled from above downward by the current, and from below upward by the globe; these resinous clouds are grey or slate coloured, according to the energy of their tension, whilst the upper and lower vitreous vapours are white and brilliant.

"When the positive vapours are numerous, the atmospheric pressure is very great; when the negative vapours predominate, the pressure is feeble. The amplitude of the extreme barometric oscillations depends upon the difference of pressures produced by the succession of positive and negative vapours.

"As the atmosphere forms a solid whole with the globe, its actions and reactions are equal; it is, therefore, indifferent in respect to the equilibrium of pressure, whether it is produced by the mere weight of the vapours, or is produced by their weight, increased or diminished by their electric action. If the ground is more pressed upon by the vitreous vapour, which is the denser, it receives a compensation equivalent to the increase of pressure by the attraction, which it experiences; in like manner, if it is less pressed upon by the resinous vapour, it receives its supplement of pressure by the repulsion which it produces.

"This compensation cannot exist in respect to the barometer, which presents only one extremity of its column to the atmospheric pressure, whilst the whole of the column experiences the same electric influence. The height of the columns in vacuo, therefore, can be in equilibrio only with the material weight of the atmosphere, and not with the complementary electric forces.

"As the material pressure diminishes with the resinous vapours, the column of mercury diminishes in height; as the material pressure augments with the vitreous vapours, the column of mercury lengthens in the same proportion. Thus the material pressure of the atmosphere increases or diminishes according to the electric state of the vapours.

"When the specific weight of the air varies by a change occurring in the temperature, the equilibrium of pressure is instantly established by an increase or a diminution in the height of the atmospheric column, and the barometer cannot be affected by it, or it is only slightly so, and that during a very short time. High barometric pressures and great depressions, which last for entire weeks, cannot recognise this cause, since they take place during uniformity of temperature as well as during its divers changes. The geographical zone, wherein evaporation is greatest, is that which possesses the greatest amount of primitive resinous vapours not saturated with electricity; the pressure there is at the mean: this takes place near the equator. In the geographical zone, where there is a uniformity in the meteorological returns,—where atmospheric condensations and dilatations are always brought about within the same limits, there are fewer accidental variations: here, again, it is near the equator that this uniformity is found. The geographical zones beyond the tropics having a lower degree of temperature, which is subject to more extensive and more sudden alterations, the vapours there experience corresponding condensations, and consequently very diverse electric tensions. The geographical zones at which the last clouds of the upper current terminate, the resinous tension of which is successively increased by the repulsive action of the globe, are those which experience the most considerable perturbations—perturbations which alternate in ratio with the masses that arrive, and the clear sky that intervenes. These zones extend from the sixtieth to the eightieth degree of latitude.

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"It follows, from all that precedes, that, according to the quantity of negative or resinous vapours which rise from a country, or which reach it from a neighbouring country, the pressure diminishes and the barometer falls, whatever be the day, the hour, and the temperature; it also follows, that the pressure increases with their condensation, which permits of a new electric distribution, and renders the lower vapours more vitreous or positive. Finally, the pressure is more variable on the level of the plains than on the summit of very high mountains, from the fact of the perturbations of the vapours interposed in the space by which they are separated: the range of the barometer, therefore, is the more regular as we rise higher above these vapours.

"*Hourly variations.*—As the presence of the sun raises the temperature every day, evaporation follows divers phases; whilst the negative vapours are rising numerously and disseminating themselves in the atmosphere, the pressure diminishes and the barometer falls. When the sun descends, and the temperature with it, the vapours condense, they conduct better, and their resinous electricity is more easily repelled; the lower vapours become vitreous, they are more attracted, denser and heavier, and the barometer rises. In consequence of the progress of condensation during the night, the inferior vapours, having become too conducteous, lose their vitreous electricity, and they are resolved into dew or hoar-frost; the upper resinous vapours act more directly, being without a counterpoise, and the barometer falls. When the sun appears again on the horizon, it first heats the upper strata of the atmosphere, dilates the vapours that had been condensed by the cooling influence of the night; those which are in the vesicular state repass to the state of elastic fluid,—they are revaporized under the repulsive influence of the resinous electricity of the globe; they carry away the resinous electricity repelled from the lower strata: they also take from them a portion of the caloric necessary to their new state. The lower vapours and atmosphere become at this moment colder,* more vitreous, more attracted to the globe, and heavier; and the barometer rises. Finally, as the solar rays heat the surface of the soil, new resinous vapours rise from its surface, and the same series of phenomena, as on the preceding day, recommences and is completed.

"*Of the pressure according to latitude.* — The upper atmospheric current is uncessingly carrying the masses of resinous vapours from the tropics towards the polar regions: from the origin between the tropics themselves, the cooling which these vapours undergo as they rise resolves a considerable quantity of them. The great conductibility of the saturated air facilitates the repulsion of the resinous electricity toward the upper strata, and the neutralization of the vitreous electricity of the inferior strata with the ground. It follows that in this zone the vapours are generally resinous, that their pressure is diminished, and that the barometer is retained below the mean. In proportion as the air loses this superabundance of moisture, the vapours become less conducteous, and the lower vapours can retain, in some degree, the vitreous tension developed by the influence of the globe. The superior and inferior vitreous strata are thicker in proportion as the tropical current is more distant from the ground; the general pressure, reduced at first by the vapours being entirely resinous, rises by the weight of the vitreous strata which form, extend, and remain. On advancing toward the poles, the tropical current approaches the earth, the space interposed between it and the ground diminishes, and with it the thickness of the strata of vitreous vapours. Then, after having arrived at a *maximum* under the empire of the thick strata of vitreous vapours, the pressure is reduced when the strata becomes thinner, and this reduction continues until about the sixty-fourth degree of latitude. The diminution of the spherical area, which receives also the tropical currents, is a powerful cause of their condensation, and their termination toward the polar circle. The march of the inferior strata of the atmosphere toward the tropics to supply the vacuum left by the ascending current, becomes also a cause of the descent of the latter and its termination. The nearer the resinous vapours of the upper current are to the ground, the more are they repelled, their pressure is diminished until, by their globular condensation and their ulterior resolution,

* Vide, in reference to this expansion, a note communicated to the Philomatic Society, p. 153, of this number of the *Elec. Mag.*—Editor.

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they restore to the pure air a greater part in the general weight; the pressure then increases, and the barometer rises, not to fall again until reaching the poles.

"*Of the pressure at the height of perpetual snow.*—On rising above the diurnal vapours and to the centre of perpetual snow, we get beyond the daily cause of atmospheric perturbations; the hourly variation of the plains disappears, to be replaced by that which depends upon the tropical current. Toward the forty-fifth degree, the diurnal vapours that are beyond 3000 mètres (1·86 miles) are most commonly become vitreous by the influence of the tropical current; the grey strata, which are the receptacles of the resinous tension, are extended below this elevation. When the diurnal vapours arrive at this height, and are interposed between the mountain-tops and the tropical current, the pressure increases and gives an ascending curve to the variation of the barometer from seven, A. M., to nine, P. M. But when, in consequence of the cooling, this vitreous stratum descends, it leaves the resinous vapours of the current to regain their predominance; the pressure then diminishes, and continues to decrease until about six or seven A. M., in the fine summer days, when it rises, as in the preceding day, by the arrival of the first vapours that ascend and become vitreous by the same superior influence. This range in pressure, although very different from that of the plains, is yet dependent on the atmosphere above these latter. If the weather is fine and the temperature high, a portion of the vapours of the day surpasses this height and modifies the pressure. If the sky is frosty and the air above the plains cold, none of the diurnal vapours reach to this height; and the pressure only incompletely represents the *maximum* of nine P. M., and the *minimum* of six A. M.; the oscillations of the barometer are vague and inconstant, like the electrical state of the superior vapours, which the current brings and changes continually. In high latitudes, the curve of great height is found again; an increase of pressure in the day, and a diminution in the night. But in those regions, when the masses of definite resinous vapours, limited in the opaque clouds, are accumulated, and when they are resolved in the midst of gales of wind and tempests, the regularity of the return of the *maximum* of the day and the *minimum* of the night is very difficult to be separated from the great and numerous perturbations which occasion the sudden arrival of these clouds and their prompt resolution.

"*Of the measure of altitudes.*—Finally, according to the vitreous or resinous state of the inferior vapours, simultaneous observations made in the plains and on elevated crests, give the altitudes too great or too little. These differences must be corrected by making the electric element enter into the co-efficient of the barometric formula, for a value, which varies according to the power and tension of the superposed strata."

Here follow a few reflections of the committee on M. Peltier's work:—

"This theory, which is the fruit of long experience, embraces, as we see, the question of variations in atmospheric pressure in all its generality. It is the first time, we think, that an attempt has been made to explain, by electric actions alone, all the movements, that agitate our atmosphere. Let us hasten to state:—M. Peltier's work is very important, and merits, in the highest degree, the attention of philosophers. Although the seducing theory of the author has not been admitted in all its parts, we cannot deny that, in a great number of cases, it explains in a very happy manner difficulties before which other theories had fallen.

"We may nevertheless inquire whether M. Peltier does not go too far in attributing the oscillations which the atmosphere undergoes exclusively to the play of electric actions; and in regarding the influence of but a single cause indiscriminately in all barometric variations?

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"The whole of M. Peltier's theory rests, as may have been seen, upon the double hypothesis, that the earth is a globe possessing a proper resinous tension, which is very energetic,—and that there exists a tropical current, constantly directing itself from the equator towards the poles, and charged like the globe, whence it is derived, with an electricity also resinous and very intense. It was possible to support the first hypothesis by experiments, and this is what M. Peltier did in another memoir; but the demonstration becomes more difficult as far as the tropical current is concerned.* It may be asked, where is the proof that the current is so energetically charged with resinous electricity? This current being found at a great height, where the temperature is very low, can be charged only with vapours of a feeble tension; it is, therefore, difficult to admit that the vapours of the current play so important a part as that which is attributed to them by the author, and that they are capable of pouring out such great quantities of water when they are made cold by their passage over certain countries. We may further remark that the zone, within which the trade-winds prevail in a sensible manner, is of little extent, scarcely extending more than 30° on both sides of the equator. Supposing that the counter-current extends a little further, it will still remain very unlikely that it can reach high latitudes, much less possess sufficient force there to be charged with sufficient vapours and electricity to be capable of producing the effects attributed to it by M. Peltier. This unlikelihood is the more striking when the extreme thinness of the atmosphere is considered in comparison with the dimensions of the earth; and when it is remarked that the tropical current can exist at only a comparatively feeble height in this very thin stratum.

Is it correct to say that, when the upper stratum of vesicular vapour evaporates completely by the heating action of the solar rays, the subjacent portion of vesicular vapour yields a part of its free heat to complete that which is necessary for the evaporation of the first stratum? This case cannot be assimilated with that in which the evaporation is determined by the existence of an unsaturated space above a liquid; here the heat of vaporization is taken from the liquid that remains, which hence experiences a cooling. In the case cited by M. Peltier, the evaporation would not have taken place had it not been favoured by an exterior source of heat; now the action of a source for determining evaporation consists precisely in furnishing the heat necessary for this effect; and the quantity of vapour that is formed is proportional to the heat communicated. Under this circumstance, the neighbouring bodies do not in any way contribute to the formation of vapours, and hence do not experience a fall of temperature. Experiment comes in support of this assertion: thus, whilst a thermometer, whose reservoir is surrounded by linen moistened in water, falls, by its exposure to an unsaturated space, in consequence of the evaporation that is

* Vide *note* previously referred to.—EDITOR.

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called into play, a similar thermometer does not at all fall when evaporation is brought about at its surface by exposure to solar rays, or by the vicinity of any hot body. In like manner also, on exposing a piece of ice or snow to a temperature above 0° (32° Fahr.), the fusion of the surface does not produce the slightest reduction of temperature in the next layers.

"M. Peltier advances that, if the ground is more pressed by vitreous vapour, which is the denser, it receives a compensation, equivalent to this increase of pressure, by the attraction, which it experiences from it. The *static equilibrium*, or this compensation between the attraction which the vitreous cloud experiences by the resinous earth, and the presumed increase in the density of the other cloud, presents difficulties to the mind, which have need to be removed.

"Is it fairly proved that if the atmospheric air were pure, the variations of pressure would be but small, if even they were sensible? To say that the variations of pressure are due *solely* to the mixture of heterogeneous substances,—to vapour,—is not this to admit the point which is to be demonstrated? . . ."

PHILOMATIC SOCIETY OF PARIS.

(*Continued from page* 62.)

July 26, 1845.—M. Peltier addressed the following note:—

"The journal *L'Institut*, in its last No. (604), gave the greater portion of the report that was made by the Committee of the Academy of Sciences at Brussels upon my memoir, entitled, *Researches upon the Causes of Barometric Variations.* This memoir, which was received by the Academy, forms part of the 18th volume of their *Mémoires des Savants Etrangers.* In this report, an error is attributed to me, which I could not have committed, and which I ask permission to rectify: it is that of attributing the cooling of the atmosphere, at daybreak, to the re-evaporation of the vapours condensed by the first solar rays.* The committee observe, and with good reason, that the rise of temperature which produces the new evaporation furnishes all the latent caloric necessary for the change of state. It is, as may be perceived, the very reproach that was made to Volta, in respect to his theory of hail. The same reproach I revived in my *Traité des Trombes*, paragraph 145, almost in the same terms; I could not, therefore, recognise in this part of the new evaporation the cause of the cold produced by the first solar rays; but in my *Traité des Trombes*, as well as in this memoir, I attribute this cooling to the re-evaporation caused by the electric tensions present,—a re-evaporation which can only find its latent caloric in the remaining vesicular vapours, and in the circumambient atmosphere. The error of attributing to me such an opinion would only arise from an insufficient compilation; I ought to have corrected it before the memoir was printed;

* Vide report in present number of *Elec. Mag.* p. 152.

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and I think that now it cannot be revived, as is apparent on reading paragraph 183.

"With regard to the tropical current, this memoir already contains numerous proofs of its existence; the mere doubt does not appear comprehensible to me. Moreover, I shall enter anew upon the demonstration, in treating upon the meteors that appear in those high regions of the atmosphere. I will reply, at the same time, to one of the doubts of the Committee, in making known the powerful electric tension to which bodies may arrive when placed in these perfectly insulating media, that are so far from the conducting strata that envelope the globe."

REVIEW.

Cosmos; a Survey of the General Physical History of the Universe. By ALEXANDER VON HUMBOLDT. Parts I. to IV.; 1 vol. post 8vo. Baillière, London.

COSMOS is not one of the many books with which the press teems, that are read to-day and forgotten to-morrow; it is a work that is read by ourselves with delight and with much profit now, and it will be referred to with no less delight by our children and our children's children. The author has grown grey in wisdom; from his youth he has been an untiring observer of nature and of nature's laws, not like some of us, in but one small segment of her wide domains, but, unlike most of us, he has looked at her on every side. Of all men, therefore, he is most fitted to take such a general glance at the universe as is here presented; and we rejoice that now, in the autumn of his life, he has given us these volumes.

The subjects of *Magnetism* and *Auroræ* are contained in No. II.; and we strongly advise our readers to make themselves acquainted with them, in order that they may learn to separate, with Humboldt, the known from the unknown. They will find there many forgotten truisms, and many important generalizations. We cannot analyze the sections without giving the major portion of them, and must, therefore, be content with making a few extracts at random:

"The hour of the day can be told between the tropics by the motion of the needle, as well as by the oscillations of the mercury in the barometer.

"There are regions of the earth where the seaman, enveloped for days in fog, without sight of the sun or stars, without all other means of ascertaining the time, can still accurately determine the hour by the variation of the dip of the needle, and know whether he be to the north or south of the port towards which he would steer his course."—Pages 188-9.

"A thousand years and more before the commencement of our era, in the dark epoch of Codru and the return of the Heraclidæ to the Peloponnesus, the Chinese had already magnetic cars, upon which the moveable arm of a human figure pointed invariably to the south, as a means of finding the way through the boundless grassy plains of Tartary."—Page 190.

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"It was not without surprise that I myself observed, among the children at play on the woody banks of the Orinocco, the offspring of native tribes in the lowest grade of civilization, that the excitement of electricity by friction was known. The boys rubbed the dry, flat, and shining seeds of a creeping leguminous plant (probably a nigretia), until they attracted fibres of cotton wool and chips of the bamboo. This amusement of these coppery children is calculated to leave a deep and solemn impression behind it. What a chasm lies between the electrical play of these savages and the discovery of the lightning-conductor, of the chemically decompounding pile, of the light-evolving magnetical apparatus! In such gulfs, millenniums in the history of the intellectual progress of mankind lie buried!"—Page 197.

"The ceaseless change, the fluctuating movements, which are observed in all magnetical phenomena—those of the dip, variation, and intensity, according to the hour of the day, and even of the night, according to the season and the lapse of whole years, permit us to suspect the existence of very dissimilar partial systems of electrical currents in the crust of the earth. Are these currents, as in Seebeck's experiments, thermo-magnetical, and immediately excited by unequal distribution of heat? Or shall we not rather regard them as induced by the position of the sun, and through the influence of his heat? Has the rotation of our planet and the accident of the different velocities impressed upon the several zones, according to their distance from the equator, any influence upon the distribution of magnetism? Shall the seat of the currents, in other words, of the electricity in motion, be sought for in the atmosphere, in the interplanetary spaces, or in the polarity of the sun and moon?"—Page 198.

". . . . . In what regards the old explanation of the horary variations of the deflection, by the progressive heating of the earth in the apparent course of the sun from east to west, it must be owned that we are here limited to the very outermost surface; inasmuch as the thermometers, now sunk in the ground in so many places, and so carefully observed, show us how slowly the sun's heat penetrates even to the moderate depth of a few feet. And then the thermal state of the surface of the ocean, covering two-thirds of the globe, is little favourable to such an explanation, when the question is one of immediate mean influence, not of induction from the aerial and vaporous covering of our planet.

"To all questions as to the ultimate physical causes of phenomena so complicated, there is no satisfactory answer to be given in the present state of our knowledge."—Page 199.

"The splendid phenomenon of coloured northern lights in the act of discharge, the conclusion of a magnetic storm—in the same way as, in the electrical storm, an evolution of light—lightning—indicates the restoration of the disturbed equilibrium in the distribution of electricity."—Page 201.

"It is not improbable that, as in the case of heavily charged and threatening clouds, and of frequent transitions of the atmospheric electricity into opposite states, it does not always come to discharges by lightning, so also may magnetic storms produce great disturbances in the horary motions of the needle over extensive circles without there being any necessity for explosions,—for luminous effusions from the pole to the equator, or from one pole to another, in order to restore the equilibrium."—Page 202.

"This connection of the polar light with the most delicate cirrus clouds deserves to be particularly mentioned; inasmuch as it shows us the electro-magnetic evolution of light as part of a meteorological process. The terrestrial magnetism here manifests itself in its effects upon the atmosphere,—in a condensation of the watery vapour which it holds dissolved. . . . . After a grand nocturnal display of the northern lights, the same streaks of clouds that had been luminous overnight, were discovered in the morning arranged in the same manner."—Page 204-5.

Part IV. contains three pages upon the *Electricity of the Atmosphere;* and also an immense mass of notes reaching to upwards of 400,—some being references to authorities, others extracts from sources referred to in the text. These notes are rich in information, and are a valuable addition to the work.

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NEW BOOKS.

Makerstoun Magnetical and Meteorological Observations, for 1841 *and* 1842 (*in Trans. R. Soc. Edin., Vol. XVII. Part I., price* 15*s.*) *Grant and Son. — Mémoire sur la Distribution de l'Electricité à la Surface de deux Sphères conductrices complètement isolées; par M. Q. Plana. Turin,* 1845, 4*to. — Observazioni; M. Zantedeschi's Observations on the Description of the Magneto-Electro-Telluric Battery, and on the Continuation of M. Palmieri's Researches on Magnetism. A leaf in* 4*to.—Cosmos; a Survey of the General Physical History of the Universe. By Alex. Von Humboldt. Parts I.—IV.*; 1 *vol. post* 8*vo.*, 10*s. Baillière, London.—Proceedings of the Magnetic and Meteorological Congress at Cambridge, in June* 1845. *London,* 8*vo. — Galvanische; Researches in Galvanism and Electro-Magnetism; 1st Part.—Electricity considered in its relation to Telegraphy. By M. Jacobi. Pamphlet,* 8*vo.*

NOTICES OF EXPERIMENTS, &c.

Specific Inductive Capacity illustrated.—"I take three equal discs of brass, arranged parallel and at equal distances from each other; the two exteriors are in communication with the ground, the third, which is between them, is insulated. You see here an electroscope consisting of a single gold leaf, suspended at an equal distance between two small brass balls, each of which communicates separately with one of the two exterior discs that I have mentioned. I charge the middle disc with a certain quantity of electricity; I cut off the connection of the other two with the ground, and as it is absolutely necessary for the success of the experiment that all things be symmetrically arranged, I take care that the gold leaf of the electroscope be at rest, by being equally attracted by the two little balls, which are in communication with the two discs, that are equally electrized by induction. At present it is the air which forms the two insulating strata, that separate the three discs; but if for one of these strata I substitute another of shell-lac, the gold leaf diverges, showing that the inducing action of the electrized body upon the disc from which it is separated by the new insulating body is become greater. Instead of shell-lac I employ glass and other strata of various insulators, and in this manner I prove that the action of induction depends on the nature of the insulating body interposed between the two conductors. [An apparatus similar to this was used by Faraday in the study of these facts."—Matteucci's Lecture, reported by M. R. Felici in "*Il Cimento,*" 2nd No. for 1845.

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Easy method of distinguishing Iron and Copper Pyrites.—Prof. V. Kobell has observed that, if a piece of copper pyrites is placed upon a zinc plate in a solution of sulphate of copper, the surface of the mineral, in the course of a few minutes, turns of a violet colour, and in a very little while becomes coated with a film of reduced copper. If in the first stage of this process the mineral is withdrawn from the fluid, its colour, after it has been washed and dried, will pass in the course of a few hours into red, to a brilliant blue, and so forth. Iron pyrites, free from copper, does not exhibit this phenomenon, whereas magnetic pyrites is affected as readily as copper pyrites is. The film thus produced, is removed by ammonia, but not by hydrochloric acid. Von Kobell appears to be of opinion that the phenomenon is due to a film of oxide, an idea which is borne out by the action of the ammonia, but not by that of the hydrochloric acid; and which is moreover at variance with the electric condition of the copper pyrites. The action is rather to be attributed to a conversion of $\dot{C}u\,\dddot{F}e$ into $\dot{C}u\,\dot{F}e^2$.—*Berzelius Jahresbericht*, 24; p. 301.—(From W. G. Letsom, Esq.)

Reduction of Metal from fused Compounds.—"Now the object of this part of my invention is to use salts or compound salts of the metals in the state of fusion; and I will here observe that the salts I have found most advantageous for the above purposes are the iodides, chlorides, and phosphates; but others may be employed, and generally those capable of holding the metals when in a fused state.

"I have also found that the salts may be used with advantage in common with other salts, as hereafter explained, avoiding the use of those salts which will decompose the salts of the metals employed.

"For example, the iodides of gold and silver may be used alone, or combined with iodide of potassium or sodium, or with the hyposulphites, and some others.

"I will now state, as an example, with reference to silver, the process I have found most perfect.

"I take of the chloride of silver 6 lbs., I fuse or liquefy the same in a convenient vessel; those I have hitherto used are of silver or of enamelled iron; but, unless the vessel be of silver, it is necessary to employ one that will support the required heat, and not be acted upon by the salts in fusion. When the salt or compound salts become fluid, I suspend a plate of silver in connection with the negative [positive?] pole of any suitable electrical apparatus, as is well understood, and the article to be plated on silvered to the positive [negative?] pole of the same arrangement, when the article will soon become coated with silver, and the desired thickness will be obtained by continuing the process. Or, I take 6 lbs. of iodide of silver, and fuse as before. If to this compound

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of silver I desire to add other salts, which is desirable in order to obtain a large quantity in a fused state for a bath, I prefer the iodide of potassium, in the proportion of 3 lbs. to 10 lbs. thereof, to 6 lbs. of the iodide of silver; and sometimes I take the salts of other metals,—as, for example, the iodides of mercury or copper,—in the proportion of 1 lb. to 2 lbs. of either of them, to 6 lbs. of the iodide of silver.

"As an example of gold, I take 20 oz. of iodide of gold; and as this salt is costly, I prefer to use with it a larger proportion of the iodide of potassium or sodium, say 80 oz. of either of them. The remaining part of the process described for silver is applicable here, except that the plate to be suspended in the fused salts will be of gold instead of silver.

"Having described as examples the best preparation I am acquainted with for gold and silver, I will observe that the process, as applied to other metals, is the same, with the exception of the salts to be employed, and is applicable with the salts of platinum, of copper and of zinc."—Mr. Alex. Parkes' Patent; *sealed* Oct. 29, 1844; *enrolled* April 29, 1845.

Solid Carbonic Acid Electric.—"On one occasion the solid carbonic acid was exceedingly electric, but I could not produce the effect again: it was probably connected with the presence of oil which was in the carbonic acid box; neither it nor the filaments of ice, which formed on it, in the air, conducted; for, when touched, it preserved its electric state. Believing as yet that the account I have given of the cause of the electric state of an issuing jet of steam and water (*Phil. Trans.* 1843, p. 17*) is the true one, I conclude that this also was a case of the production of electricity simply by friction and unconnected with vaporization."—Faraday, *Phil. Trans.* for 1845, Part 1. p. 160.

Self-charging Gas Battery.—This arrangement was described in a paper by Professor Grove, at the Royal Society, June 19th,—at the British Association, June 21st,—and was shown at Mr. Gassiot's *Soirée Electrique*, June 28th.—The glass cells to contain the acid water are open; the hydrogen tubes, of which one is in each cell, all enter a common glass tube, which is above them, like the hydraulic main in a gas work; one end of this tube is closed, and the other is connected with a generator of hydrogen, similar to that of Dobereiner's apparatus; by this means the supply of hydrogen is constantly maintained. The platinized plates are in each tube, and other plates, properly connected, are immersed in the acid liquid of the cells, which, being exposed to air, keeps up the supply of oxygen. Mr. Grove's experiments show that the most invariable action may be relied on for years.

Electro-Culture.—The Rev. Mr. Sydney, in his lecture on this subject,† alluded to certain experiments by Prof. Solly, then in progress at the Chiswick Gardens. In June last, Prof. Solly stated to the British Association, that the seeds of barley, wheat, rye, tur-

* Vide *Elec. Mag.* Vol. I. p. 129. † Vide *Elec. Mag.*, Vol. II., No. 9, p. 44.

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nips, and radish, when exposed to a feeble current of electricity, came up sooner and were healthier than similar plants unelectrified. Other seeds gave opposite results; of experiments on 55 different seeds, 21 were in favour, and 10 against the electric current; 25 were neutral. In all these cases, the seeds were planted between a zinc and a copper plate buried in the soil. On the whole, he felt doubtful whether the effects observed were really due to electricity. I visited the crops, in company with Mr. Solly, in August; and, from the general appearance of the experiments, was compelled to agree with him in pausing before giving the electric current credit for having been the cause of the occasionally favourable appearances presented by some specimens. I paid another visit to them on Sept. 12th. The voltaic currents are supplied by a plate of zinc and one of copper, about 6 in. square, buried on each side of the seeds; I am not inclined to regard the results of the experiments as decisive, either for or against the current. Mr. Solly suggests that as the lower parts of the plates are in moister earth, the greater portion of the action may have passed *beneath* some of the seeds during germination. Of four patches of potatoes, the first is growing in the ordinary way; the second has wires 12 ft. high, with the lower ends in the ground, and the points directed to the heavens; the third has wires stretched a few feet from the ground, from which are suspended other wires pointing to the crop; the fourth has buried plates at each end; if the tops are any indication, the latter may be a little the best crop; but nothing certain can be known till the product is weighed. A patch is planted with barley in the usual way; another patch is surrounded with wire after Mr. Forster's fashion, which wire is connected to two wires that are carried up poles, and terminate skyward in several points. Both crops have been sadly beaten down by the rain, and no apparent difference exists between them.—C. V. W.

Omnia.—The House of Commons have, this last session, voted 5839*l.* for the expense of the Magnetic Observations at home and abroad.—The Galvanic Shower Bath is one of the last offerings to the popular mania of the day; it is a zinc vessel fitting tight to the head, like a hat; the water, in its upper part, has a fall of an inch or two; two or three strips of copper are attached to the lower part, for conscience sake, and it is hence termed *Galvanic.*—The Society of Arts offer the Gold Medal for any important useful application of Electricity to the Arts. The Society have in view the probable economical application of electricity, galvanism, electromagnetism, or magneto-electricity, to chemical manufactures, to the production of motive power and of light.—The same Society presented an Honorary Testimonial to Mr. C. Varley, jun., for his "Portable Electrical Machine."*—On May 6th, 1845, was sealed, an English patent, by "Charles Wheatstone, of Conduit-street, Esq., and William Fothergill Cooke, of Kidbrooke, near Black-

* Vide *Elec. Mag.* Vol. II. p. 54.

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heath, Esq., for Improvements in Electro-Telegraphs, and in Apparatus relating thereto; part of which improvements are applicable to other purposes."—Dr. Miller takes the Chair of Chemistry, at King's College, London, vacant by the death of Professor Daniell.—The Emperor of Russia, in token of the great estimation his Majesty entertains of Mr. Harris's ability in the construction of Lightning Conductors for shipping, has presented him with a handsome porcelain vase. It is twenty inches high, and eighteen in diameter; the body is blue, the base burnished gold; it bears the following inscription: "To W. Snow Harris, Esq., from H. I. M. Nicholas Pawlowitch, Emperor of all the Russias, January, 1845." —Galvanic Rings have, for some time, been at a discount.—Mr. Beatson has obtained the musical sound by the passage of a charge from a Leyden jar.—The final experiment between Monza and Milan,* was the shock given by an induced current, which included the earth between these two towns in its circuit.—A New York journal talks of a mode of communicating with England by means of the Electric Telegraph.—The value of the platinum obtained from the Russian mines, since 1819, when they were first worked, is 1,120,000*l.*—We have heard that the Electro-Cultural experiments at Findrassie,† have turned out to be anything but successful.—A patent for Scotland was sealed, Jan. 7th, 1845, by "Alexander Bain, of Charlotte-street West, in the county of Middlesex, for Improvements in Apparatus for Ascertaining and Registering the progress and direction of ships and other vessels through the Water, and, for Ascertaining the Temperature in the holds of Ships or other vessels, for taking their soundings at sea, and in Apparatus used in Light-houses."—The *Giornale del Commercio di Firenza* states, that at the meeting of the Medical and Physical Academy, on April 12th, Dr. J. Cervelleri, professor of anatomy at Naples, made a most learned communication, in which he asserts that the stone can be dispersed by means of electricity.— We buried zinc plates at the one end, and iron fire bars at the other, of some alternate rows of potatoes; the potato-arms were somewhat more luxuriant, but the crop was not improved.—M. Matteucci has lately discovered some curious facts relative to the electricity of the torpedo, which he hopes may soon enable him to explain why the back and the belly form the extremities of the pile.—The elegant spire of St. Dunstan's-in-the-East is now protected from lightning; we have applied a three-fourth inch copper rod, and have adopted the same general arrangements as at the Royal Exchange.—The mass of magnetic observations, accumulated within the last three years, and which remain for discussion, amount to nearly two millions.—M. Juvioli planted beans in a jar varnished with lac, they did not germinate until after the usual time, which he attributes to the electrical state of the vessel.—Mr. Dell has repeated the experiments on potatoes and on corn, *secundum artem;* the potatoes were improved—the corn was not.

* Vide *Elec. Mag.* Vol. I. p. 593. † Vide *Elec. Mag.* Vol. II. p. 48.

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ELECTRICAL MAGAZINE.

JANUARY, 1846.

CXXIX. *Description of a Colossal Hydro-Electric Machine, and on some of the Phenomena attending the Production of Electricity by Steam.* By W. G. ARMSTRONG, Esq. Read before the British Association at Cambridge. Communicated by the Author.

IT was not my intention, on arriving at Cambridge on the present occasion, to have communicated a paper to the British Association; nor had I in any degree prepared myself for so doing; but the salutary influence which that Institution is calculated to exercise in stimulating its members to exertion in the cause of science, has produced its natural effect upon me, and led me to contribute a mite in furtherance of the objects for which we are associated.

The powerful steam electric apparatus (or hydro-electric machine, as I have named it) which has now been exhibited for a considerable time at the Polytechnic Institution, in London, has demonstrated that effluent steam is, beyond all comparison, the most efficient means of producing *frictional* electricity, with which we are acquainted. Extraordinary, however, as is the power of that machine, it has recently been entirely eclipsed by a similar apparatus, upon an improved construction, which has lately been made in Newcastle, and sent out to the United States of America; and I venture to hope, that a brief description of this machine, with some account of its effects, may not be altogether uninteresting to this section.

The boiler of the machine is not larger in its external dimensions than that at the Polytechnic Institution,—that is to say, it is 6½ feet long, exclusive of the smoke box at the

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end, and 3½ feet in diameter. It is supported upon glass pillars for the purpose of insulation, and is heated by an internal fire, which is so arranged as to produce a much more rapid evaporation than is effected in the Polytechnic machine, but the chief difference between that machine and the new one, lies in those parts of the apparatus which are more immediately concerned in the production of the electricity, viz., *the escape apertures and condensing pipes.*

I have no hesitation in saying, that I regard it as an *ascertained fact*, that the evolution of electricity in the hydro-electric machine is due to the *friction* sustained by particles *of water* (not steam) in passing through the discharging apertures; and that steam itself is merely the vehicle by which the aqueous particles are expelled, and by which the friction is occasioned. By no other explanation is it possible to account for the prodigious influence which is exercised by the form of the escape orifice,—by the material against which the current is rubbed,—by the presence of water in the issuing steam,—and by the condition of such water with respect to extraneous substances with which it is combined.

In the machine at the Polytechnic Institution, there is a single row of steam jets, or escape apertures, which extends from one end of the boiler to the other. To each of these jets the steam is conducted through an iron pipe, which may be called the condensing pipe, in which a portion of nearly *pure* water is precipitated by the refrigerating action of the external air, upon the same principle as the moisture of the breath is condensed, when we blow through a tube or a musical instrument. The water which is thus deposited in the condensing pipe is swept along by the current of steam, and dashed, in the form of a highly divided spray, through the discharging aperture, against the sides of which it undergoes a violent friction, and receives, in consequence, an electrical excitation, which it communicates to the entire cloud formed by the steam as it issues from the aperture.

Now I have found it to be a matter of extreme nicety, so to adjust the quantity of water deposited in the condensing pipes as to obtain the maximum excitation of electricity. If, on the one hand, there be an excess of water, then two results will ensue, each tending to lessen the electricity produced.

1st. The mean density of the issuing current of steam and water is increased, which causes the velocity of efflux and consequent energy of the friction to be diminished.

And 2nd. The ejected steam cloud is rendered so good a conductor by the excess of moisture, that a large proportion

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of the electricity manifested in the cloud retrocedes to the boiler, and neutralizes a corresponding proportion of the opposite element.

On the other hand, if the quantity of water be too small, then, although each particle of water may be excited to the fullest extent, the effect is rendered deficient, in consequence of the *insufficient number* of aqueous particles which undergo excitation.

Now the method of condensing by contact of the external air, which I adopted in the Polytechnic machine, is liable to this defect, viz., that whenever the steam is discharged from such a number of jets as to cause the density of the steam in the boiler to diminish very rapidly, the pipes cease to condense the requisite quantity of water, because, as the density of the steam diminishes, its temperature also diminishes, and the pipes do not cool down with sufficient rapidity to keep pace with the declining temperature of the steam.

To remedy this defect, I have contrived a method, which has been adopted in the American machine, of condensing, by the application of water to the exterior surface of the pipes; and this is effected by placing a trough of water beneath the row of condensing pipes, and suspending from each pipe a sufficient number of cotton threads, to lift, by capillary attraction, just as much water as is required for the cooling of the pipe. By increasing or diminishing the quantity of cotton, the condensation may be regulated with the utmost nicety, and so effectively does this method keep down the temperature of the pipe, during the subsidence of the temperature of the steam, that twice or three times the number of jets that could formerly be used may now be employed.

There are 140 condensing pipes and jets in the American machine, and they are ranged in two horizontal rows, one above the other, on the same side of the machine.

I greatly regretted that unavoidable absence from Newcastle prevented my seeing this machine after it was fully completed. I had only one opportunity of witnessing its action, and it was then in a very imperfect condition; but even on that occasion it charged a Leyden battery, consisting of thirty-six jars, containing in the whole 33 feet of coated surface, to the utmost degree that the battery would bear, upwards of sixty times in a minute, being equivalent to charging nearly 2000 feet of coated surface in one minute, which is at least twenty times greater than the utmost effect that could be obtained from the largest glass electrical machine ever constructed.

The sparks were not longer than those which were ob-

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tained from the Polytechnic machine when it stood in the open air; but the rapidity with which they succeeded each other was three or four times as great as in the other machine.

By passing the current of electricity produced by the machine through the superposed coil of fine wire in a Callan's apparatus for inductive effects, and making an interval in the conductor, so as to cause the electricity to pass in a succession of sparks, I obtained a secondary current in the thick wire answering in every respect to an alternating voltaic current; insomuch that, when I attached a piece of iron to one end of the wire, and a steel file to the other, and rubbed them together, a slight but *constant* deflagration of the metals was produced, and I had every reason to believe that, with longer and better insulated coils, the same effect would have been exhibited in a much more conspicuous manner.

I was also enabled, by means of the secondary current, to excite a small electro-magnet sufficiently to cause it to sustain a piece of iron wire about an inch long, and one-tenth of an inch in diameter, and the effect would probably have been greater if the successive pulsations of the current in opposite directions had been less rapid than they were.

I shall now advert, in a very few words, to certain phenomena connected with the production of electricity by steam, which are not at present very generally known, and which, in my opinion, are possessed of very considerable interest.

It is invariably found that, when hydro-electric machines are first tried, their power is extremely feeble, and that they require to be used for a considerable time before they attain their maximum efficiency. The reason of this I connect with another fact, which is well known to engineers, though not perfectly understood, viz., that new boilers have always a tendency to "*prime*," as it is termed in technical language, that is, to throw out with the steam a portion of *unvaporized* water. Now it is of the utmost importance in hydro-electric machines that the water ejected with the steam, and exposed to friction in the escape aperture, *should be pure*; and, for that reason, that it should be supplied by *distillation*, and not derived direct from the water in the boiler, which is never sufficiently pure for the purpose. Hence, therefore, whenever the boiler "*primes*," even in a degree imperceptible to common observation, the action of the machine is impaired, and it may generally be presumed, when a hydro-electric is not performing up to its proper power, that impurities, detrimental to the effect, are passing from the boiler into the steam passages. This will explain why the action of the machine is influenced by the nature of the water from which the steam

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is generated; some waters having a greater tendency to prime than others, and containing substances more or less injurious to the operation of the machine;—and it also explains why it is advisable frequently to draw off the residual water, and wash out the boiler.

Professor Faraday has shown that the electricity of the steam cloud may be reversed by introducing oil of turpentine into the steam passages, and he attributes the effect to the particles of water becoming coated with a film of turpentine, and thereby virtually converted, so far as friction is concerned, into entire particles of that substance;—but there are substances which are *soluble*, and which cannot, therefore, like turpentine, diffuse themselves over the surface of the water; but which, nevertheless, are equally effective in reversing the electrical state of the boiler and steam cloud. For example, if a piece of cotton steeped in a weak solution of acetate of lead be inserted in the condensing pipe, the electricity of the steam, which in general is positive, will be changed to negative; and thus water, which in a state of purity stands at the very head of *positive* electrics, falls low down in the scale of *negative* electrics, when it holds in solution a minute quantity of acetate of lead.

It is perfectly surprising how extremely small an admixture of some substances is sufficient to produce this extraordinary transmutation from one electrical state to the other. For instance, when the condensing pipe is of brass instead of iron, the steam cloud is *positively* electrified the same as in ordinary cases; but if the pipe, previously to being used, has been immersed in very dilute nitric acid, then *negative* electricity, instead of positive, will be evolved by the steam cloud, notwithstanding that the pipe may have been washed with clean water subsequently to its immersion in the acid; and, as far as my experience goes, a brass condensing pipe which has been thus prepared *never* fully regains the condition necessary for the production of a powerful development of positive electricity in the steam, until it has been treated with an alkali, in order to eradicate every vestige of the metallic salts formed by the combination of the acid with the substance of the pipe.

The effects are also singularly influenced by the material of which the condensing pipe is formed. Thus glass, lead, tin, copper, and iron are each effective in a different degree, and I have every reason to believe that, in all these cases, the variation is due to minute quantities of extraneous matter acquired by the condensed water acting chemically or mechanically upon the material of the pipes.

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These are curious facts, and are well worthy of a more careful investigation than my professional avocations have permitted me to bestow upon them.

CXXX. *Note on the Liberation of Electricity, which takes place in the Experiment of the Burst Bladder.* By J. Duprez, Professor of Physics at the Athenæum and at the Industrial School of Gand. From "Bulletins of the Brussels Academy." Vol. xii. No. 8.

(Translation.)

The experiment of the burst bladder, which is introduced in lectures on physics in evidence of the pressure which the air exercises on bodies, is well known; but the point which, as far as I am aware, does not appear to have been hitherto observed, is a liberation of electricity which occurs in the same experiment. This liberation may easily be proved by employing, as the receiver of the bursting bladder, a glass cylinder from 20 to 30 centm. (8 to 12 in.) high, and 10 to 15 centm. (4 to 6 in.) in diameter. Half way up, the side of the cylinder is pierced with a hole, in which is cemented a copper conductor, 3 or 4 decim. (12 to 16 in.) long. At one extremity of this conductor, that which is placed in the interior of the cylinder, and nearly in the axis of the latter, is screwed a small ball, also of copper, 3 or 4 cent. ($1\frac{1}{2}$ in.) in diameter, and the other extremity is bent so as to be able to be plunged into the mercury of a small cup, terminating the stem of a gold leaf electroscope, and which is intended to establish a perfect communication between that instrument and the conductor.

The apparatus being thus prepared, a membrane of bladder is stretched upon the cylinder, and, after it has been allowed to dry, the cylinder is placed upon the plate of an air pump, care being taken to place the conductor in communication with the mercury contained in the cup of the electroscope; the vacuum is then made. At the moment when the membrane bursts, the gold leaves of the electroscope, even though their length exceeds 7 centm. (3 in.), are projected against the armatures of the instrument by the electricity that is set free. It sometimes happens in this experiment that the membrane, presenting too much resistance, does not burst when the vacuum is carried as far as the air-pump permits, but then a gentle blow given to the membrane with the slightly-roughened end of a glass rod is sufficient to make it burst. In all cases,

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the electricity with which the electroscope is charged is positive.

It is well known that, in England, Mr. Armstrong and Mr. Faraday have obtained electricity by allowing air to pass out of a vessel in which it had been strongly compressed. According to Mr. Faraday, this electricity is to be attributed to the friction which the vapour of water, contained in the compressed air, and condensed by the expansion of the latter, exercises against the side of the escape orifice, or against bodies placed in its course. It was while desirous of repeating some of Faraday's experiments, but with air subjected merely to the pressure of the atmosphere, that the idea occurred to me of making the experiment of the burst bladder. It was, therefore, natural, at first sight, to regard the electricity, that is liberated in this experiment, as produced by the same cause as that to which the electricity observed in Faraday's experiments is due; and this with the more reason as I had already, in another experiment, obtained electric signs by allowing the air to return again into a glass globe in which a vacuum had been made. However, experience and reflection soon taught me that it was not so. In fact, it is not in the friction exercised by the particles of water contained in the air against the side of the conductor placed in the interior of the recipient of the burst bladder that the cause of production of electricity resides; for if, in order considerably to diminish this friction, this part is replaced by a very fine copper wire soldered to the conductor, we observe that the electricity with which this wire becomes charged at the moment when the membrane bursts, is equally strong with that which is obtained with the part of the conductor terminated in a ball. On the other hand, the large opening of the receiver no longer permits us to entertain the opinion that the electricity would arise from the friction exercised against the receiver itself. Finally, it is difficult to suppose that the quantity of water produced by the sudden condensation of the vapour contained in the air, is sufficient to produce by friction an electricity so powerful as that attested by the divergence of the gold leaves of the electroscope.

It is elsewhere, therefore, than in the friction of the particles of water, either against the conductor or against the receiver, that we must seek for the cause of this electricity. With this view the following experiment may be made:—One end of a small piece of tin-foil, a few centimetres in width, is fixed, by means of a little gum arabic, to the membrane of bladder that is stretched over the receiver, and near to the edge of the latter; the other extremity of this tin-foil is tied

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round a small bent rod of copper, which is plunged into the mercury cup of an electroscope. The conductor of the receiver is then made to communicate with a second electroscope; after which the vacuum is made in the receiver. At the moment when the membrane bursts, the two electroscopes diverge powerfully; the one which communicates with the conductor of the receiver is charged, as usual, with positive electricity, whilst the other, namely that which communicates with the membrane, is charged, on the contrary, with negative electricity.

It follows, from this experiment, that the cause of the liberation of the electricity resides at the entrance of the receiver; and as the parts of the membrane of bladder, which remain adhering to the side of the receiver, possess negative electricity, it is to be presumed that those which are carried away and projected into the receiver possess the contrary electricity, and that they communicate this electricity to the conductor which they meet. This supposition is verified directly by experiment. A membrane of bladder is stretched over the receiver, and, by means of the slip of tin-foil already described, it is put in communication with an electroscope. A little rent is produced in this membrane, and, by means of a small pair of insulated pincers, a part of this membrane is rapidly torn away at the moment the leaves of the electroscope present a negative divergence. If the torn part of the membrane be presented to a second electroscope, the leaves of the latter immediately diverge with positive electricity. This latter experiment evidently shows that the electric effect obtained in the experiment of the burst bladder, is produced by the destruction of the molecular attraction of the parts of the membrane of bladder, and that it is analogous to the electrical effects which occur in the cleavage of certain bodies. —[A.]

CXXXI. *On the Vibratory Movements which the Transmission of Electric Currents or their Exterior Action determine in Bodies, and essentially in Iron.* By Prof. DE LA RIVE. "Archives de l'Electricité," Vol. v. p. 200, No. 17. July, 1845.

(TRANSLATION.)

IN 1837, Mr. Page had obtained a musical sound, by bringing a flat spiral, that was traversed by an electric current, near to one or both poles of a powerful horse-shoe magnet. Every time that he made or broke the circuit in the wire of the spiral,

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he heard a prolonged sound in the magnet.* In 1838, M. Delezenne succeeded in like manner in producing a sound by rapidly rotating the soft iron armature in front of the poles of the iron horse-shoe magnet of a Saxton's machine; he obtained the same effect by rotating a magnetized short iron bar between the branches of a piece of annealed soft iron. In the first case, it was the magnet; in the second, the soft iron, which gave the sound.†

In 1843, when occupied in experiments upon the chemical effects of induced currents, I frequently perceived that the piece of soft iron that is placed in the interior of the helix, in order to increase the intensity of these currents, experienced powerful vibrations, by the effect of the alternate passage and interruption, in the wire of the helix, of the current produced by a powerful Grove's element. I made at that time a few experiments, by placing within the bobbin plates and rods of soft iron, as well as one of the branches of a tuning fork, also of soft iron, and fixed vertically by its bent part upon a sounding board. In addition to a series of strokes, corresponding to the alternate passages and interruptions of this current, certain musical sounds were heard, which appeared to me to be similar to those that the same pieces of soft iron presented when they were struck directly.

I communicated these results to the *Société de Physique et d'Histoire Naturelle de Genève,* on March 21, 1844. In January 15, 1845, I communicated to the same society some experiments, from which it appeared that the passage of a discontinuous current through a bar, or, still better, through a soft iron wire stretched on a sounding board, determines vibrations which produce a very powerful musical sound. I added, that the same effect is produced, but in a much less degree, by the passage of the discontinuous current through steel and all other metals. Since that time, I have seen, in *L'Institut* of January 8, 1845, that Mr. Marrian, of Birmingham, has, in like manner, succeeded in producing a very decided sound in a bar of soft iron, placed in the interior of a helix, at the moment when he completed, as well as at the moment when he interrupted, the voltaic circuit in the wire of his helix.‡ I have also learned, that, in 1844, Mr. Gassiot made the same experiment on a large scale with the powerful apparatus that he possesses, and that he obtained sounds of remarkable intensity. Mr. Beatson, in the April number of *The Electrical Magazine,* states that he has obtained the same kind of phe-

* *Bibl. Univ., New Series,* vol. xi. p. 398. † Ibid. vol. xvi. p. 406.
‡ Vide *Elec. Mag.* Vol. I. p. 527.

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nomena for about two years, and that he had no knowledge of the analogous results obtained by Mr. Marrian, except from the *Literary Gazette* of November. Mr. Beatson adds, that he has recently obtained a vibratory motion, by making the current pass along an iron wire, as well as along wires of other metals; but in the latter case, the effect was much more decided. These facts are altogether similar to those which I communicated to the *Société de Physique et d'Histoire Naturelle*, on January 15th of the present year. It was on this account that, as soon as I had knowledge of these things, I transmitted an extract of my work to M. Arago, to be communicated to the *Académie des Sciences;* this extract was inserted in the *Compte Rendu des Séances de l'Académie* of April 28, 1845.* I have also, in the present number of *Les Archives*, given the article from *L'Institut* relating to Mr. Marrian's experiment, as well as a translation of Mr. Beatson's notice, which appeared in *The Electrical Magazine.*

I now pass on to my own experiments, which I will put forth in detail :—

FIRST PART.—*General Study of the Vibrations produced by the Exterior Action of Currents upon Iron.*

The apparatus that I have employed for rendering the current discontinuous are of various kinds. I will call them Rheo-tomes, to use an expression already employed by Mr. Wheatstone in his memoir on *Several Instruments for Determining the Constant's of a Voltaic Battery.*† One is a simple toothed wheel, which is turned with various velocities round an axis passing through its centre, and the teeth of which successively strike against an elastic metal plate; every time that the plate and the tooth of the wheel are in contact, the circuit is established; every time that the contact ceases to exist, the circuit is interrupted. A register adapted to the apparatus indicates the velocity of the rotation impressed upon the wheel, and consequently the number of times in a second that the current has been made and broken. Another apparatus, somewhat similar to that which I have just described, differs from it only in that the contact between the teeth of the wheel and the elastic plate is more prolonged, and that it can give to discontinuous currents an alternately contrary direction. Of all rheo-tomes, the one which has seemed to me preferable for this class of experiments, is an apparatus in which the circuit is closed by means of two brass needles communicating metallically together by a common axis, upon which they are fixed by their centre, and to which a clock movement gives a

* Vide *Elec. Mag.*, Vol. II. p. 44, No. 9. † Vide *Elec. Mag.*, Vol. I. p. 203.

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rotatory motion; these two needles, in their rotatory motion, plunge each into a capsule filled with mercury, and emerge alternately with such velocity that they open and close the circuit about thirty times in a second. Finally; I have sometimes, also, made use of the induced current as a discontinuous current, either that which occurs in the wire rolled round a bobbin, parallel with the wire that transmits the inducing current, or that which occurs in the latter wire itself. The oscillating motion produced by the alternate magnetization and demagnetization of a bar of soft iron, placed in the interior of a helix, whose wire is traversed by an electric current, has also procured me the means of alternately making and breaking contact between two platinum conductors, forming part of the circuit that I desire alternately to open and close. In order to make this apparatus work, it is necessary to have a small voltaic pair, independent of those which compose the pile, whose current is to traverse the circuit submitted to experiment; this small pair is intended to produce the oscillating movement, and thus to render the current of the pile discontinuous.* The rheo-tomes, as well as the voltaic apparatus, whose current they were to render discontinuous, were always placed in a different room from that in which the bodies were to be put in vibration; there was even, between these two rooms, a third room, traversed by the copper conductors employed for transmitting the current. These precautions were taken, in order to prevent the noise of the rheo-tomes from interfering with the observer in the perception of the sounds arising from the vibratory motions due to the action of the electric current.

I first studied, in a general manner, the vibratory motions, which are determined by discontinuous currents in bodies placed in the interior of the helix, whose wire they are traversing. Helices of different diameters, pieces of soft iron of different forms and different dimensions were subjected to experiment. Finally, even iron filings were placed in the interior of the helix.

Whatever be the form and size of the pieces of soft iron, two sounds are always distinguished; one is a succession of blows or rather of shocks, more or less dry, very analogous, in some cases, to the noise made by rain in falling on a metal

* The greater portion of these apparatus were constructed by Mr. Bonijol, with the intelligent care which this distinguished artist takes in the construction of all instruments that come out of his hands. Mr. Bonijol was also of great assistance to me in the series of experiments into which I was led, and of which I am about to give an account; independently of the services which he has rendered me by his accuracy in observations, he has more than once facilitated my task, by the ingenious manner in which he has known how to realise, by the construction of an apparatus, the experiments that I had in view.

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roof; these blows exactly correspond to the alternations of passage and interruption in the current; the other sound is a musical sound, of a greater or less degree of sharpness, which corresponds to the sound that would be given by the mass of iron by the effect of transverse vibrations, or to the harmonics of this sound.

Rods of soft iron are decidedly those which give the most intense sounds under similar circumstances; if the current is strong, there is no great difference between the intensity of the sound rendered by a rod of hardened iron, and a similar rod of annealed iron; but the sound is always much more feeble with a steel rod, it is also much sharper and more *metallic*.* A solid cylindrical mass of iron 10 c.m. (4 inches) in diameter, and weighing 10 k.g. (22 lbs.), placed in the interior of a bobbin, surrounded by a thick copper wire covered with silk, and making five or six turns round the bobbin, produced a very clear and brilliant musical sound; it was the same as that which it gave out on being struck. I also obtained a remarkable sound by placing in the interior of the same bobbin, a tube of soft sheet iron, very thin, and of a little less diameter than that of the bobbin, and which, being supported on a wooden block, communicated by means of this block with a sounding board. More than one sound was then heard; it is probable that the air was put in vibration in the interior of the tube. Finally, a rod of soft iron, 3 metres (10 feet) in length and 1 centim. (.39 inches) in diameter, fixed by one of its ends on the sounding board, gave, in addition to the fundamental sound which resulted from its transverse vibrations, other sounds equally perceptible, and very well sustained.

In all these cases, it was remarked that the sound had greater intensity when the helix acted on the middle of the soft iron rod, or tube, or bar. There was also greater intensity in the sound, when the helix acted on the part of the rod situated at a quarter of its length from its free extremity.

The results which I have just described differ notably from those obtained by Mr. Marrian. This philosopher found that soft iron, tempered steel, and magnets produced the same sounds; I have found, on the contrary, great differences between the sounds produced in these different cases. These differences are probably due to Mr. Marrian's having confined himself in his experiment to closing and opening the circuit once only, so that he had a sound of but short duration; whilst in mine, the circuit being opened and closed alternately several

* The word *metallic* appears to me a very good expression for a clearer, drier, and more brilliant sound, such as is given by a metal wire, compared with that given by a string of cat-gut.

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times in succession—thirty times, for instance, in a second —I obtained a sustained sound more suited to observation.

Wishing to become well acquainted with the nature of the vibration that soft iron experiences, when it is thus successively magnetized and demagnetized by the influence of a discontinuous electric current, I took a disc of very thin sheet iron, and, holding it by a brass rod fixed in its centre, I introduced it into the interior of the helix; I had taken care to give it a somewhat smaller diameter, in order that its edges should not touch the sides of the bobbin, around which the wire of the helix was rolled. This disc immediately manifested a tendency to place itself in such a manner that its plane, instead of being perpendicular to the axis of the helix, was parallel to it; in fact, it placed itself in this manner immediately it ceased to be retained by the rod; but, as the existence of the rod opposed its taking this position, it began to vibrate with great violence, on account of the changes of form which were determined in it by the tendency impressed upon it by the current, joined to its thinness and its elasticity. This kind of effect, which is entirely mechanical, and which is exercised on the mass as a whole, must be carefully distinguished from the molecular vibration determined by magnetization. It is the same when a thin plate of soft iron, or one of the branches of a tuning-fork, is introduced into the helix; the mechanical action that is exercised by the current on the entire mass of iron which it has magnetized, determines vibrations in it similar to that which every other mechanical action would determine. To guard against these vibrations, the piece of iron subjected to experiment must be either fixed very solidly, or must be placed in the very position in the interior of the helix that it would take, if abandoned to itself, by the effect of the action of the helix upon it. Thus, if it is a rod of soft iron, it must be either fixed solidly by its two extremities, or it must be thrust in in such a manner that its middle is exactly in the middle of the helix; for, if allowed to be free or floating upon mercury, in the interior of a helix whose axis is horizontal, it would be thus placed by the action of a current traversing the wire of the helix. It is the same for pieces of iron having other forms.

To return to the disc—I have succeeded in making it produce a small very sharp noise, by preventing its executing its vibrations together, and placing it in the middle of the helix, perpendicular to its axis. I placed it in the same position, but in another manner, by making it float on water, the level of which exactly corresponded with the middle of the helix; I had slightly greased its surface, in order that it might be thus

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sustained horizontally in the water. No more sound was heard, but a slight undulatory motion was perceived in the water around its circumference; this movement was much more pronounced on the central surface of the water, which was exposed by cutting away the centre of the disc so as to make a ring of it; a series of concentric undulations were then seen on the water, the centre of which coincided with that of the ring.

But the most remarkable effects of motion that I obtained, were produced by employing very fine iron filings, from iron as soft as possible. I placed them on a disc, which formed the bottom of a cylindrical box of pasteboard, of such a diameter that it entered exactly into the large bobbin of 12 c.m. (4¹ in.) in diameter. The current of a powerful Grove's pile (its element of 6 inches square) traversed the wire of the helix in a discontinuous manner. The helix itself was so placed that its axis was vertical. At the moment when I approached from above to the disc on which the filings were with the opening of the bobbin, the filings were agitated; one part was then carried toward the edge, and another toward the centre of the disc. I passed the disc within the helix, and as soon as I had passed the first wire the filings stood erect, and formed a multitude of small pyramids, all distinct from each other, and which commenced turning about, and flying from the centre, in order to approach the edges; the height of the pyramids and their movement acquired their maximum when the disc was in the middle of the helix. By thrusting it in further there was a slight diminution, and at the bottom of the helix a part of the filings had returned to the centre.

In order to the success of this experiment, which, by-the-by, is remarkable for its elegance, it is necessary that the succession of discontinuous currents should not be too rapid, and that the time during which each current passes, should be longer than that during which it does not pass. Thus it succeeds better with the mercury rheo-tome than with the others; with the toothed-wheel rheo-tome the experiment goes on very well, if there are only from twenty to forty interruptions per second; it also succeeds with eighty, but not so well; at one hundred and sixty it ceases, and there is no effect upon the filings. These different results, according to the velocity with which the currents succeed each other, probably arise from the fact that even the softest iron has still a little coercitative force, and that it requires a certain time to be magnetized and demagnetized. In this respect, I compared together *iron*, *nickel*, and *cobalt*, prepared by hydrogen, and I found that the nickel powder still manifested motion for a velocity of suc-

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cession of currents at which that of iron ceased to manifest any; and that cobalt powder, on the contrary, ceased to manifest any before that of iron, which very well accords with what we elsewhere know of the relative coercitative force of these three metals.

Instead of placing the iron filings on a pasteboard disc, I placed them upon an iron disc; in a similar manner they formed small pyramids as soon as the disc was thrust into the interior of the helix, but these pyramids did not manifest any motion. This is probably due to the fact that the particles of the iron disc, which serve as a base to each of the pyramids, cannot, on account of their mutual cohesion, obey the tendency of separating from each other, and that, therefore, the particles of filings which form the pyramid remain motionless, on account of the strong attraction which connects them together, and which unites to their bases those that are in contact with them. We have merely to place between the filings and the surface of the disc the thinnest sheet of paper, in order to permit the movement of the filings to be accomplished, but with certain differences. Indeed, when in the interior of the bobbin there is a cylinder of soft iron, and the iron filings are placed on a paper disc, with which the upper surface of this cylinder is covered, contrary to what occurs when there was no mass of iron, we see the filings carried towards the edges of the bobbin, if the surface on which it rests is level with the upper part of the helix; if this surface is on the middle of the helix, the filings are seen to fly from the edges, forming small vertical pyramids. In this case they are even projected with great force, to a height of 8 or 10 c.m. (3 or 4 in.); this also occurs when there is not a mass of soft iron, if there are many iron filings in the interior of the helix.

The phenomena are the same when the filings are placed on a copper disc, instead of being placed on one of pasteboard. By employing a tinned disc, there is a slight movement in the filings, whilst there is not with a disc of sheet iron; if the movement is feeble, this is probably due to the extreme thinness of the tin coating by which the iron of the disc is separated from the filings.

I placed within the interior of the bobbin a tube of soft iron, which entered and just filled it, and the sides of which were only 1 m.m. ($\frac{1}{25}$ in.) thick; the strongest current, whether continuous or discontinuous, was without the slightest action upon filings placed interiorly, as in the preceding experiments. Only when the disc on which they were placed was brought within the distance of 1 c.m. ($\frac{2}{5}$ in.) of the upper extremity of the tube, they rose in an agitated manner, and

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suddenly passed entirely from the interior to the exterior of the tube, against the surface of which they placed themselves in rays. A brass tube, similar in all respects to the one of iron, did not modify the action of the current upon the filings, as did the latter; the same was the case with a sheet iron tube of the same dimensions, which was obtained by rolling into a cylinder a sheet of iron, and simply rivetting it to the two edges of the plate, without soldering. It would be interesting to know whether a tube of soft iron, split length-wise, but not from one of its extremities to the other, would behave like the entirely continuous tube, or like that which is cut along the whole length.

For the present, I shall not insist upon the other phenomena presented by the action of the helix upon soft iron, when the current by which it is traversed is continuous; I shall return to this in a memoir on magnetization, which will form a sequel to this. I will simply confine myself to remarking that the results which I have just put forth are in no degree modified when the helix is placed in the interior of a soft iron tube, when it enters fitting accurately; the said tube is not magnetized by the action of the current.

I, in like manner, confine myself to simply pointing out, without dwelling upon it at present, a very pronounced sound which the bobbin gives forth when the helix of copper wire that surrounds it is traversed by a powerful discontinuous current.

Second Part.—*More Special Study of the Vibratory Motions in Iron Wires.*

§ 1. *Description of the Apparatus.*

After the general examination of the divers phenomena of vibrations to which the actions of discontinuous currents gives rise, I pass to the more particular study of the vibratory motions, which are determined in iron wires by the exterior influence of these currents or their transmission through the wires themselves.

With this view I constructed an instrument similar to a monochord, on which could be extended, parallel to each other, six strings, each of about two métres (6¹ ft.) in length; these strings are fixed by their extremities to brass pegs, which permit of their being placed either separately or simultaneously in the voltaic circuit. They may be more or less tightened by means of a screw, the thread of which moves the piece onward, to which one of their extremities is fixed; or else a weight may be made use of to produce the tension, by passing

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over a pulley the wire that is to be stretched, or else a cord of catgut fixed to its free extremity.*

The iron wires were all thrown into vibration, sometimes by the action of the discontinuous current traversing the wire of a helix rolled round a bobbin, in the axis of which the wire passed without touching, at other times by means of the discontinuous current traversing the iron wire itself. For the sake of brevity, I shall designate these two modes by the simple words, *action of the helix, action of the transmitted current.* I employed different helices, some with an extreme diameter of only 1 c.m. ($\frac{2}{5}$ in.). others of 3 or 4 c.m.; the copper wire, covered with silk, of which they were formed, was 2 m.m. (·078 in.) in diameter in the smaller, and 3 m.m. (·118 in.) in the larger; the convolutions, hermetically sealed against each other, formed five or six superposed strata; the helices were 8 or 10 c.m. (3 or 4 in.) in length.

§ 2. *Action of the Helix on the Wire.*

On moving the helix, by means of a small movable support, so that it acts successively on the different parts of the wire, we very quickly perceive that its action is stronger when it is placed so as to act on the two portions that are nearest to the middle of the wire, rather than on the middle itself. There also exist other portions of the wire on which the action of the helix produces a stronger sound than on the others; these are the parts neighbouring to the nodes; they also change their place with the absolute length of the wire. The wires that vibrate best are very soft iron wires, from 4 to 2 m.m. (·15 to ·078 in.) in diameter; if they are larger they become rods, which give less full sounds, and if they are thinner, they give more feeble sounds; and this is for the most part due to the fine wires acquiring a very feeble magnetism by the action of the helices.

The same wire was thrown into vibration by three helices placed either one close after the other, or at greater or less distances; the same current traversed them successively; the sound was much stronger; and it was equally so, whatever the direction of the current was in each helix, which proves that the portion of wire that is lodged in the interior of the helix, is magnetised in an independent manner; and that, even when the momentary magnetism that is impressed upon it is con-

* The instrument that I have just described was constructed by Mr. Mussard, a distinguished artist of our city, who has made some very interesting researches upon the vibration of steel wires. Mr. Mussard has also been kind enough to assist me in the exact determination of the tones that were given out by the strings that I subjected to experiment, and the tensions that they underwent.

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trary to that acquired by the portion which precedes, and that which follows, the vibratory motion resulting from it is not modified. It was not the same when, instead of making successive helices act upon the iron wire, I submitted the same portion to the action of two discontinuous currents sent in contrary directions, and circulating separately in two wires rolled together round the same bobbin, so as to form two superposed helices; the vibratory movement was then nothing, since the two contrary magnetizations, impressed at the same time on the same part of the wire, must have become neutralized.

§ 3. *Action of Transmitted Currents on Wires.*

The sounds, that are determined by the transmission of the discontinuous currents through wires, depend essentially, with regard to their intensity, upon the force of the current and the diameter of the wires. Five Grove's pairs gave me a very strong sound with a wire 2 m.m. (·078 in.) in diameter, and 1·9 m. (3½ ft.) in length. The sound is strengthened, if the wires are shortened; they must not, however, be so shortened that the current heats them so as to make them begin to become red. It is the same with these effects as with the calorific effects of the current; the maximum of action occurs when the resistance of the conductor is equal to that of the pile. Since, therefore, the current traverses metal wires,—namely, good conductors,—we must use piles composed of but very few pairs, but with large surfaces; in fact, two Grove's pairs of 25 c.m. (11·8 in.) square have constantly given me very intense effects, whilst a pile of twenty small Daniell's pairs with very high tension, gave me no perceptible effects, neither did the passage of electricity derived from a powerful electrical machine.

§ 4. *Relations and Differences between the Sounds produced by one Mode, and those produced by the other.*

Whichever of the two modes is employed to produce the vibration, provided that the wire be suitably stretched and the succession of currents does not surpass a certain velocity, besides a series of blows corresponding to the alternate interruptions and re-establishments of the circuit, a mass of sounds is heard, the effect of which is surprising, and which greatly resembles that produced by several church clocks vibrating in the distance. The sound that is best heard is the sharp octave of the fundamental sound, then its sharp fifth; its double and triple sharp octaves are sometimes heard, as are also the double and triple sharp octaves of its fifth, and the double sharp octave of its third; finally, the fundamental

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sound itself is heard with greater or less distinctness. There is sometimes considerable difficulty in perceiving it in the midst of this clatter of different sounds; this is perhaps on account of its deepness. It would even seem that, among the sounds produced, there are some, which are not harmonics; thus, for example, the fourth is in some cases heard. In order to make all these musical sounds disappear, we have merely to touch the wire with the finger in any one point; however, when the vibration is very powerful, the fifth is still preserved on touching the wire slightly at a third of its length, and the double sharp octave is preserved on touching it at the fourth. When all the harmonic sounds cease to be heard, we perceive nothing but the noise or series of shocks following each other with a greater or less degree of rapidity, which are determined by the passage of the discontinuous current in the wire; their intensity is in no degree affected by the obstacles which prevent the wire from vibrating.

There exist, however, certain differences between the sounds that are due to the action of the helix and those which arise from the direct transmission of the current through the wire. The fundamental sound produced by the transmission of the current is generally a little lower, which is due to the slight heating produced by the current in traversing the wire; for this difference does not exist if the wire is sufficiently large not to be heated, or if the tension remains the same, notwithstanding the heating. The sounds produced by the transmission of the current are more easily deadened than those that are due to the action of the helix; they are also prolonged for a much less time after the cessation of the causes, that gave rise to them. On the other hand, they seem to be more numerous, and to embrace a greater range of the musical scale. These differences are probably due to the same cause, namely, that the vibratory motion is disseminated in the whole extent of the wire in the case of the direct action of the transmitted current, because this current traverses it from one end to the other, whilst it is concentrated on the part of the wire submitted to the action of the helix, in the case in which it is this action which produces the vibrations.

As regards intensity, the direct action of the transmitted current exceeds that of the helix, when the current is sufficiently strong to raise slightly the temperature of the wire; it therefore exceeds it more easily when the wire is fine, it being always understood that we are here speaking only of soft iron wires very well annealed. It is no longer the same if the wires have been hardened; the sound is more feeble when it is derived from the transmitted current; moreover, the

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harmonic sounds are not heard nearly so well; the noise derived from the succession of blows is frequently the only noise that is perceptible. The difference is still greater with steel wires; the intensity of the sounds due to the action of the helix is, in this case, infinitely superior to that of the sounds due to the transmitted current, even when the wires are heated by the passage of the current. The three kinds of wire submitted to experiment had the same diameter of 2 m.m., (·078 in.), and always the length of about 2 metres (6·5 ft.); they were also equally stretched. I should not neglect to remark, that, even under the action of the helix, the hard iron wires and those of steel gave weaker sounds than did the well annealed wires; I suppose them all to be placed exactly in similar conditions, of the same diameter, of the same length, and equally stretched.

It would appear to follow from the experiments that I have just related, that the action of the helix is more suited, than that of the current transmitted directly, to surmount the coercitative force of the metal in the production of vibratory motions, and that to this cause must be attributed its effect being proportionately stronger upon steel and upon hard iron wires than upon those of soft iron. It follows, as a matter of course, that the noise derived from the succession of the interior shocks of the particles, experiences the same variations of intensity as the harmonics themselves.

In what has preceded, I have supposed that the current was transmitted directly, either through the iron wire itself, or through the wire of the helix, for when it traverses successively the two wires, so that its intensity is the same in one as in the others, the vibratory motion that it produces, by the intervention of the helix, is, in all cases, much stronger than that produced by its immediate action on the iron wire.

§ 5. *Influence of Tension.*

I have already said, that, in order to have intense sounds, a *suitable* tension must be given to the wire, that is to say, that for each wire there is a tension which gives the strongest sounds.

A soft wire, 1·9 m. (6¼ feet) in length and 1 m.m. (·039 in.) in diameter, gave the most intense sounds, when it was put into vibration by the helix, under the tension of 1 k.g. (2·2 lbs.); the intensity rapidly diminished, in proportion as the tension gradually increased from 1 to 8 k.g.; it also diminished under an increasing tension from 8 to 16 k.g., and it did not experience any sensible diminution from 16 to 26 k.g.—a tension under which the wire broke. Harmonic sounds were no longer heard, but merely a succession of shocks singularly enfeebled.

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An iron wire, similarly softened, of the same length as the preceding, and of 2 m.m. (·078 in.) in diameter, also put into vibration by the helix, gave similar results; beginning at a tension of 4 k.g., which produced the maximum of intensity, the sounds diminished in proportion as the wire was more stretched. Under a tension of 60 k.g. (132 lbs.) the shocks alone were heard; the harmonic sounds had disappeared. In this experiment, as in the preceding, we had merely to give a sudden increase or diminution to the tension, in order to produce immediately a corresponding increase or diminution in the intensity of the sounds.

'Instead of varying the tension of the wires directly by weights, I found it more convenient and exact to vary it by turning to a greater or less degree the peg of the sounding board, to which the adjusting piece is fixed, that carries the extremity of each wire, the other extremity of which is held by a fixed clasp. A monochord, whose wire was put into unison with the iron wire as it was stretched, enabled us to know, in each case, the tension of this wire, after having determined it directly in a single case.

In the experiments that follow, I have made a helix, traversed by the discontinuous current, to act upon each wire.

The results that are obtained with the transmitted current differ very little; only it is not so easy a matter to appreciate them accurately, on account of the heating that the wire undergoes, in consequence of the passage of the electric current.

Soft wire 1·3 m.m. (·05 in.) diameter, and 1·576 m. (5·15 ft.) in length. Tension giving the strongest sounds, 10·50 k.g. (23 lbs.) (134 vibrations per second).

Tension giving the weakest sounds, 12·10 k.g. (26½ lbs.) (144 vibrations per second).

N.B.—A very sharp harmonic sound is also heard with the latter tension; it is the strongest the wire can sustain without breaking.

Soft wire, 2·2 m.m. (·085 in.) diameter, 1·576 m. (5·15 ft.) in length. Tension giving the strongest sounds 36·66 k.g. (80¾ lbs.) (141 vibrations per second).

Tension giving the weakest sounds 76 k.g. (167·6 lbs.) (204 vibrations per second).

Soft wire, 2·85 m.m. (·11 in.) diameter, and 1·576 m. (5·15 ft.) in length. Tension giving the strongest sounds, 35·50 k.m. (78¼ lbs.) (124 vibrations per second).

It was not possible, with this latter wire, to obtain a sufficiently powerful tension to make all the harmonic sounds disappear.

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The following is another experiment made upon two wires exactly similar, but one *soft* and the other *hard*, both being 1·576 m. (5·15 ft.) in length, and 1·8 m.m. (·07 in.) in diameter.

Tension giving the maximum effect, { *Soft*, 26 to 53 k.g. 57¼ to 116¾ lbs. *Hard*, 29 to 57 k.g. 64 to 125¾ lbs.

The tension of 26 and that of 29 k.g. correspond for each of the two wires to 81 vibrations per second; that of 53 and of 57, to 107 vibrations.

Tensions giving the most feeble harmonic sounds. { *Soft*, 70 k.g. (154¼ lbs.) *Hard*, 77 k.g. (169¾ lbs.) } 122 vibrations per second.

I should add that the tension at which the harmonic sounds obtain their maximum of intensity, as well as that at which they entirely disappear, are not for the same wire perfectly invariable; they depend, especially the second, within certain limits, on the absolute intensity of the current, and on the more or less prolonged tension to which the wire has been previously subjected.

I believe I am able to conclude, from the facts I have just put forth, that the existence of a tension giving the maximum of intensity to harmonic sounds is due to there being, for a given wire, a tension which is that at which the sound given forth, when it is put into vibration, is the strongest. On the other hand, the interior shocks of the particles against each other, which are here the cause of the vibratory motion, are stronger as the particles are nearer, and as, consequently, the tension is less. We are assured of this by damping the harmonic sounds, so as to hear only the series of shocks; the latter have their maximum of intensity when the wire is not stretched, and when it is simply subjected to the action of its own rigidity. But in this state of no tension, the wire, even when the interior shocks are very strong, can give forth only very feeble harmonic sounds; it must, therefore, be stretched, so as to increase the force of its sounds, without diminishing too much the intensity of the shock, the tension giving the maximum effect is thus obtained. If it is passed, the shocks diminish in intensity, and such a tension is finally attained, that they have no longer the necessary force for putting the wire in vibration.

It would be interesting to seek, by the employment of different modes of making the cord vibrate, what the tension is, which gives the maximum effect for each. In a practical point of view, it would not be without utility to know what tension must be given to a wire, in order that it may give forth

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the fullest sound; and, consequently, what is the tonic sound that it is most fitted for giving forth.

But a phenomenon occurs here which greatly complicates the study of the question,—it is the simultaneous production of several harmonic sounds. When the tension is increased beyond that which gives the maximum of intensity, these sounds do not all diminish in the same proportion, and they do not all disappear equally. It is not always very easy to distinguish the order in which they vanish; but when, after having arrived at the tension at which they have all disappeared, this tension is very gradually diminished, the order in which they successively reappear may be very well determined. This determination is not very easy; however, after having made it a great number of times, I arrived at results which appear to me worthy of confidence.

I select the following from among several experiments. The soft iron wire was 1·576 m. (5·15 ft.) long, and 2 m.m. (·078 in.) in diameter; it was thrown into vibration by the helix. It was somewhat strongly stretched, in order that the harmonic sounds might have almost totally disappeared. Setting out from this tension, it was very gradually unwound, and successively stretched:—

The triple sharp octave of the fundamental sound, produced by $\frac{1}{8}$ of the total length of the wire.

The double sharp octave of the quarter, produced by $\frac{1}{6}$.

The double sharp octave of the fundamental sound, produced by $\frac{1}{4}$.

The sharp octave of the fifth, produced by $\frac{1}{3}$.

The sharp octave of the fundamental sound, produced by $\frac{1}{2}$.

Finally, the fundamental sound, produced by 1.

Occasionally between the second and third sound, produced by $\frac{1}{6}$ and $\frac{1}{4}$, the double sharp octave of the third, produced by $\frac{1}{8}$, the total length of the wire, was heard.

When, on loosening the wire, the new sounds appeared, the old ones did not disappear, so that the more there are, the more difficult it becomes to distinguish those which are just added from those which already exist. The sound which is generally the most difficult to distinguish is the fundamental sound; the best method of obtaining it, is to pass the wire briskly from the tension that corresponds to the minimum of intensity of harmonic sounds, to that which gives the maximum.

In certain cases, when the tension has been very strong, the fourth sharp octave of the fundamental sound has been heard, namely, the sound given by $\frac{1}{16}$ the total length of the wire, as well as the sound given by $\frac{1}{12}$, namely, the triple

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sharp octave of the fifth, and the sound given by $\frac{1}{10}$, the triple octave of the third.

A very remarkable thing is, that, when the wire is put into vibration by the direct transmission of the current, it is the fifths that are the most easily heard; thus the sound produced by $\frac{1}{12}$ the length of the wire, and that given by $\frac{1}{6}$, are at first perceived very well; the octaves are not heard until after these sounds. The reverse occurs when the wire is put into vibration by the helix; in this case the series of octaves predominates. It might not be impossible that this latter fact arises from a circumstance altogether accidental; the length of the helix that I employed was 10 c.m. (4 in.), and that of the wire 1·576 m. (5·15 ft.), it would follow that the part of the wire put into vibration in the interior of the helix was $\frac{1}{16}$ of its total length. However, as, in this case, the other harmonic sounds, as well as the octaves, were heard, I ought not to attach to this circumstance more importance than it merits; it, however, appeared to me right to mention it.

Whatever may be the case, it appears to me to follow, from the experiments I have just related, that, when a metal wire is put into vibration by the vibratory motion produced either by the transmission itself of an electric current, or its exterior action, this vibratory motion is the more feeble as the particles are more distant; that the more parts, however small, are comprehended in the subdivision acquired by the wire in its vibrations, the more feeble is the vibratory motion. On this account it is that, at first, only $\frac{1}{16}$ of the length of the wire is put into vibration, then $\frac{1}{12}$, then $\frac{1}{6}$, $\frac{1}{5}$, $\frac{1}{4}$, $\frac{1}{3}$, $\frac{1}{2}$; and it is only when the vibratory movement has attained its maximum of intensity, that the wire vibrates in its whole length.

I shall not dilate more upon this subject, although I am convinced that it is susceptible of interesting applications to questions of molecular physics. This manner of putting bodies into vibration by oscillations impressed upon the particles, not only upon those at the surface, but upon those of the whole mass, by means of an action exercised upon each of them directly, and not by mechanical exterior action, appears to me fitted for throwing light upon the molecular constitution of bodies, if, with this view, we employ the laws of acoustics, as Javart has already done, and as M. Wertheim has but very recently done with great success. A thing worthy of remark is, that when particles are made to oscillate by the transmitted electric current, or by the influence of an exterior current circulating along the wire of the helix, it is

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never the fundamental sound—namely, that which the wire would naturally give out by the effect of an exterior mechanical action—which is best heard, or which is heard alone. Does this fact originate in that the cause which puts the particles into vibration acts here (especially when this cause is the transmitted current) upon each of them individually, instead of acting upon the mass as a whole, by an action exercised only upon a portion?

§ 6. *Comparative Effect of different Rheotomes employed to render the Current discontinuous.*

The rheotome that I most frequently employed is that with mercury; it has the advantage of producing the most sustained musical sounds and shocks that are least dry. The toothed wheel produces an effect precisely contrary. I think I may be able to attribute this difference to the fact, that, in the mercury rheotome, the interruption of the current is more gradual and not sudden, as in that of the toothed wheel, on account of the small portion of mercury which the needles always draw with them when they emerge from it. The difference is the more sensible when the wire is put into vibration by the transmitted current, than when it is by the helix. With the rheotome in which the current is interrupted by motion impressed upon a part of the conductor, forming a portion of the circuit by means of the alternate magnetization and demagnetization of a piece of iron placed in the interior of a bobbin, the sounds are softer than with the wheel, and less than with the mercury. For producing the vibrations, I also used a strong induced current,—either that which occurs in the induced wire, or that which occurs in the inducing wire itself. The second gave the stronger sound, which is natural, seeing that when employing the induced current in the inducing wire, the inducing current itself is in an equal manner made use of. In both cases the discontinuous currents were alternately made to pass in contrary directions; this produces no change in the results, of which I convinced myself by other means, in a direct manner, by employing a rheotome, which transmitted the current itself of the voltaic pile, sent alternately in contrary directions.

I return to the toothed wheel. I passed successively through the same wire, and through the helix that surrounded it, the current rendered discontinuous by the toothed wheel. With eighty interruptions per second, the sharp fifth was very distinctly heard; the octave was better heard with 160; with 320 the intensity of the sounds notably diminished; with 640 the harmonic sounds were no longer heard, but merely a

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rumbling due to the rapid succession of the shocks of the particles excited by the alternate passage and interruptions of the current. When, after having made the toothed wheel progress with great velocity, the motion is relaxed, the sharp octave first begins to be perceived, and then the fifth. The order in which the sounds successively re-appear, is, in general, the same as that which is obtained when the wire is gradually unwound, after having been wound up to the point at which all harmonic sounds cease to be heard.

A frequent characteristic of the employment of the toothed wheel, as a rheotome, is the predominence of the sound produced by the successive shocks over the harmonic sounds arising from the vibrations impressed upon the wire by these shocks. For a velocity less than thirty-two per second, this sound is a series of sharp or metallic blows, so much resembling those that result in the rheotome itself from the blows of the metal plate against the teeth of the wheel, that every one who hears them for the first time would think that it is these very sounds that are transmitted through the conductors; but it is easy to convince one's self that this is not the csse, by more than one means, and, in particular, by deadening the latter, which does not prevent the former being equally well heard. When the velocity becomes greater, the sound arising from the succession of shocks in the wire becomes continuous, and produces a certain degree of sharp rumbling, according to the velocity of the toothed-wheel, but always perfectly identical with that which this wheel produces. With a very great velocity, the two sounds resemble the blowing of wind.

This experiment shows that the blows produced in a metal, either by the direct transmission of a current, or by the influence of the helix, whose current traverses the wire, are due to a friction of the metallic particles against each other, perfectly similar to that which occurs between the teeth of the metal wheel and the elastic metal plate, against which they alternately experience a powerful rubbing.

This noise is the same, as to intensity, in wires of different lengths and diameters: it is, in like manner, given by solid masses of iron in any form. Indeed, what is very curious is, that very fine iron filings also make it heard. To obtain this, a few filings are placed at the bottom of a small cylindrical box, which is introduced into the interior of a bobbin whose axis is vertical, being kept in a horizontal position by a rod planted in its centre. The bobbin is surrounded by the wire of the helix which is traversed by the current, rendered discontinuous by means of the toothed-wheel. By thrusting the iron filings more or less into the interior of the helix, they

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are agitated so long as there are not more than eighty interruptions per second, and, at the same time, they are heard to give out a sound, perfectly similar to, although less intense than that given by the toothed-wheel. At 160 interruptions per second of the current, the filings cease to manifest motion: but they render the same sound as the toothed wheel.

Fine powders of iron, nickel, and cobalt, obtained by the action of hydrogen on the oxide of these metals, also render the same sound; that of cobalt not quite so good; that of nickel a little better than that of iron.

In order that the sound given out by the filings or the fine powders be very pronounced, it is essential that they should form a layer of 2 or 3 m.m. thick on the disc by which they are sustained, and that they be introduced into a helix, whose diameter is not too large (4 or 5 cent.).

It follows, from what has been said, that, in order to study the series of shocks to which the passage or the exterior influence of the discontinuous current gives rise, it is preferable to use a toothed wheel as a rheotome; whilst, in order to obtain the harmonic sounds due to the vibratory motion impressed upon the wire by the succession of these shocks, it is better to use a mercury rheotome, or any other by means of which the interruption of the circuit is less sudden.

§ 7. *Modifications given to the Effects of Vibration by a Magnetization or other Electric Currents than those which produce the Vibratory Movement.*

Having submitted a soft iron wire 1 or 2 m.m. in diameter to the action of the helix, I passed the current by which the helix was traversed through the iron wire itself. A new sound was immediately heard, independently of the ordinary sounds given out by the wire; this sound, which was much sharper and more metallic, was altogether analogous to that which would have been produced by a well-tempered steel rod, that might have been struck exteriorly. I convinced myself that this effect did not arise from a diminution of the current due to its transmission through the iron wire; and, with this view, I passed it through a similar wire, but not placed in the interior of the helix, still continuing to transmit it, at the same time, through the wire of the helix. The effect was the same as if there had been no iron wire in the circuit.

It appeared to me probable that this new sound is due to this, that, on the one hand, the current transmitted directly, and on the other, the magnetization tending to give the par-

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ticles a different position, there is between them a shock of a particular nature, in the conflict of the two forces, to the action of which they are subjected at the same time.

In order that I might study this phenomenon more accurately, I passed a continuous current through the iron wire upon which the discontinuous current that traversed the helix acted. A new and very sharp and metallic sound, like that of which I have just spoken, was heard. The same occurred when I allowed a continuous current transmitted through the wire of the helix to act upon the wire traversed by the discontinuous current. But the effect was less pronounced, because one part only of the wire traversed by the discontinuous current was submitted to the permanent action of the helix, whilst, in the preceding case, all the part of the wire upon which the discontinuous current of the helix acted was traversed by the continuous current. The continuous current, in both cases, was not so strong as the discontinuous; it produced no heating effect upon the wire which it traversed. An iron wire, of which I had already made much use, suddenly gave the same sharp sound when it was traversed by the discontinuous current, even when it was not submitted to the permanent action of the helix. I then perceived that it was magnetized; it had ultimately acquired this magnetization, probably by the effect of the simultaneous actions of the helix and of the transmitted current. Having taken another similar wire, but not magnetic, I no longer had the sharp sound; the ordinary sounds alone were heard.

The close identity of the sounds obtained in either case would give rise to a suspicion that the passage of the current and the magnetization impressed upon the particles a new and similar permanent position. But, then, how are we to explain that the simultaneous and discontinuous action of the transmitted current and of the helix gives rise to a sound which seems to result from an opposition in the direction, which these two actions tend to impress upon the particles?

In order to know on what to depend, I passed a continuous current through a soft iron wire, which was traversed by a discontinuous current; this wire, which was giving forth harmonic sounds, immediately ceased to render any sound, and produced no noise at all; when I ceased to transmit the continuous current without interrupting the discontinuous, the sounds and shocks were immediately heard again. I thus, several times in succession, placed the iron wire, which was constantly traversed by the discontinuous current, in the circuit of a continuous current, and I alternately removed it;

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sounds were no longer heard as soon as it was placed in the circuit; the sounds re-appeared as soon as it was taken out.

Thus the continuous current and permanent magnetization do not impress upon the particles the same position. In fact, if the discontinuous current exercises no action over the wire traversed by the continuous current, it is because it finds the particles already arranged by the effect of this latter current, just as it tends itself to arrange them; whilst, if it does give one sound more (the sharp metallic sound) to the wire subjected to permanent magnetization, it is because it tends to impress upon the particles an arrangement different from that which results to them from this magnetization.

In the preceding experiment, the continuous and the discontinuous currents went in the same direction in the iron wire, and they both circulated equally well, as could be easily proved by seeing that the quantity of iron filings which the wire attracted was more considerable when it was traversed by both, than when it was traversed by only one of them—especially by the discontinuous.

In order that the sound should be altogether null, a certain relation is required between the force of the two currents and the diameter of the wire. If the discontinuous current is the more powerful, the sound is not altogether null, but enormously weakened.

When the currents pass in contrary directions, a portion only of the currents traverses the conductor; since the circuit, in consequence of the arrangement of the piles, may be established independently of this conductor. The sound, under these circumstances, is merely weakened.

I made the analogous experiment, by using the exterior action of the current. With this view, and by means of a helix traversed by a continuous current, I magnetised the wire in a permanent manner. I then passed a discontinuous current through the wire of a similar helix, placed after the former, and acting upon the wire—not upon the same part, but on a neighbouring part. The sound was not null, but was much more feeble than it was when the helix traversed by the discontinuous current acted alone; and, above all, it was very different. It was like the noise of iron filings, or sand sliding upon card or, better still, upon glass.

To make the experiment more complete, I operated upon the wire with a bobbin, surrounded by two similar wires. I then transmitted a continuous current through one of the wires, and a discontinuous through the other. The sound became much more feeble, compared with what it was when

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the discontinuous current acted alone; but the diminution of intensity was much greater when the two currents traversed in the same direction, and consequently tended to impress the same magnetic polarity on each extremity of the part of the iron wire submitted to their action, than when they traversed in contrary directions. In this case, the sound was a little less weakened, and a somewhat sharper sound was heard.

The sound was reduced, and made entirely to disappear, by simply connecting with a metallic conductor, the two ends of that wire which is not traversed by the discontinuous current. But this effect differs from the preceding, in that it is probably due to the effect of the current, which traverses the other wire, being annulled by the induced current, which may then be established in that wire. If, instead of transmitting this induced current through a simple exterior conductor, it is made to pass through the wire of a second helix placed beside the first, and acting upon the same wire, a very marked although more feeble sound is obtained, than when the discontinuous current acts alone, and when no induced current is established, either in the second wire of the bobbin or in the wire of the second helix.

The sound, with regard to its nature, differs slightly, according as the induced currents pass at the same time in the same or in different directions, in the second wire of the first bobbin, or in the only wire of the second; this depends on which extremity of these two wires is placed in mutual communication.

I made also the preceding experiments on iron wires of different diameters; they all gave me the same result—only, when the diameter is considerable, in order to annul the sound resulting from the passage of the discontinuous current, it is necessary to pass a more powerful continuous current through the wire itself. We would further add that, since the new sound resulting from the action of the discontinuous current upon a soft iron wire submitted to the permanent action of the helix, or from the discontinuous action of the helix upon a wire traversed by a permanent current, is altogether similar to that which well tempered steel wire would give, the consequence seems to follow, that the permanent actions in question, give to the particles an arrangement analogous to that which they have in the steel, and which is impressed upon them by tempering.

The results that I have just described, establish, in a positive manner, what the preceding experiments appear already to have demonstrated, that the passage of an electric current, as well as magnetization, determines in iron a displacement

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and a new arrangement of the particles; and, moreover, that this arrangement is permanent, and that it endures as long as the passage of the current and the magnetization endures. They, moreover, establish that the arrangement of the particles that is determined by the passage of the current, is not the same as that determined by magnetization, because the effect of the same cause, for example, of the passage of the discontinuous current through the wire, or of this passage through the wire of the helix, is not the same when the permanent arrangement of the particles of iron is produced by one of the actions, and when it is produced by the other.

§ 8. *Influence of Temperature on the Vibratory Motions.*

In the course of my experiments, I had frequently observed a gradual swell produced in the sound during the first moments that the current traversed an iron wire, whose vibratory motions I was studying. I also observed that the swell only occurred, when the transmitted electric current was sufficiently powerful to heat the wire in a notable manner, without, however, bringing it to a red heat. This circumstance induced me to examine the effect of a powerful heat applied directly to the metal wires, whilst they are submitted either to the action of the transmitted current, or to the exterior action of the helix.

With this view, I arranged beneath the wire a small, long, and narrow metal box, filled with alcohol, to which I set fire, so that the wire, which was gradually heated, arrived and maintained itself for five or ten minutes in the state of complete incandescence. The wire of 1 m.m. in diameter was stretched by a weight of 1 or 2 k.g., so that, notwithstanding the increase of length which it acquired from its heating, its tension might remain constant.

The sound was produced by the transmitted current; it was heard to increase notably in the first moments of its heating; but, when the wire had become whitish-red, it singularly diminished. It then resembled a noise produced by iron filings, or grains of sand sliding upon glass. It was sufficient to remove the flame for a moment by blowing it, for the sound to become immediately more intense. This intensity increased throughout the duration of the cooling, until the moment when the wire had arrived at its primitive temperature, namely, that of the chamber; the sound itself then regained an intensity less than that with which it had just been manifested, but greater than that which it had had at the commencement. During the duration of the cooling, there are moments

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when the sound resembles that which would be given by a multitude of small bells sounding together. It is not necessary to heat the wire traversed by the current along its whole length; the same effects occur, although in a less degree, if it is only heated to a certain extent.

It appears, therefore, that an elevated temperature diminishes in the wire its capacity of giving out sounds by the effect of the transmitted current, but that on the other hand this capacity increases, independently of the absolute temperature, by the effect of a less pronounced amount of heating and cooling.

This curious effect of the motion of heat in a wire cannot be attributed to the simple separation or approach of the particles, because when this separation or approach occurs by the effect of a simple mechanical cause, no augmentation of intensity is perceived resulting from the increase of tension, at least none so sensible as those which result from the elevation of temperature.

I heated, in a similar manner, a wire put into vibration by the action of the current transmitted through the helix. The part of the wire in the very interior of the helix cannot be heated, but simply the portions projecting on each side. The sound diminished in intensity during the heating; it became very feeble when the heated parts of the wire had become incandescent. It constantly increased in intensity during the cooling, until the moment when, the temperature of the wire having become what it was at the commencement, it regained its primitive intensity. There were not, as in the preceding case, any sensible phases of augmentation in the intensity of the sound during the heating and cooling.

This augmentation in the facility possessed by the transmitted current of putting the particles of iron into vibration—an augmentation resulting from the movement of heat in this metal, made me at first presume that this same cause might also increase the intensity of the vibrations that I had believed to be produced in other metals by the transmission of the discontinuous current. This is exactly what happened. A copper wire, a plated wire, a platinum wire, 1 or 2 m.m. in diameter, and 1 metre long, produced more sensible sounds, in which the harmonics were very well distinguished, when they were alternately heated and cooled whilst they were transmitting the electric current. The action of the heat alone determines no perceptible sound, either in this or in the preceding case.

It follows from the researches contained in this memoir, 1st. That the discontinuous action of the electric current de-

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termines, in the bodies which it traverses, a succession of vibrations, whose intensity is much more pronounced in magnetic bodies, and especially in bodies such as soft iron, susceptible of acquiring transient magnetism.

2nd. That magnetic bodies may in like manner be put into vibration by the exterior action of a helix, whose wire is traversed by a discontinuous current.

3rd. That certain circumstances, such as a greater or less tension, the divers means employed to render the current discontinuous, the influence of heat, may render the vibrations, and consequently the sounds, produced by either mode, more or less intense.

4th. That, according to the degree of tension and the velocity with which the discontinuous currents succeed each other, certain sounds are heard in preference to others, and that those which correspond to the most numerous sub-divisions of the wire, disappear the last, in proportion as the wire is more stretched.

5th. That the sounds in question arise from the oscillatory motions of the particles and of the shocks which occur between them, in consequence of the two actions already pointed out, an effect which may very easily be verified on fine iron filings.

6th. That, in bodies traversed by a continuous electric current, or subjected to permanent magnetization, the particles are differently arranged from what they are when the bodies are in their natural state, and that this different molecular state is connected with the greater part of the phenomena produced by the transmission of the electric current, such as the development of heat, chemical action, induction, and physiological effects.—[A.]

CXXXII. *On Currents of Electricity produced by the Vibration of Wires and Metallic Rods.* By Mr. Sullivan. —(Abstract from *Phil. Mag.* Oct. 1845.)

The author had an idea that electricity was connected with molecular motion:—

In order to test the accuracy of my views, I took a flat piece of board, about two feet long, and morticed two uprights, each three inches high, into it, about eighteen inches apart. I then took a piece of No. 15 brass wire, and soldered it to a similar piece of iron wire, and stretched it tight over the uprights, so as to produce, when set in vibration, a musical note; the ends of this compound wire were then connected with an exceedingly delicate galvanometer, similar to that used by Melloni in his researches on radiant

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heat; when this was set in rapid vibration, a slight deviation of the needle took place, the direction of the current being from the iron to the brass, as in a similar thermo-electric circle; this was not, however, always the case, but seemed to depend upon the kind of vibration, though I could not discover how. That this effect did not depend on any heat produced is quite certain, as the current ceased the moment the vibration ceased. I could not always produce a deviation, and very often, when there was a current produced, it was scarcely appreciable until I increased the oscillation of the galvanometer-needle by frequently breaking contact. On one or two occasions I noticed a deviation equal to that produced by heating the junction of the two wires to dull redness.

He increased the mass of metal in order to obtain a greater quantity of electricity. He says:—

I took a bar of antimony five inches long, three-eighths of an inch wide, and one quarter of an inch thick, and soldered it to a similar bar of bismuth; to the ends of this compound bar copper wires were attached, which communicate with a galvanometer. When this was set in vibration by striking it with a piece of iron, as a file, suspended from a piece of string, results similar to the former were obtained; at times considerable deviations were thus obtained, whilst very often no deviation whatever would be observed, the whole effect seeming to depend on the manner in which the vibrations are propagated along the wire. The results were, on the whole, much more satisfactory with this arrangement than the former. I could also detect the nature of the vibrations occurring in such a bar much better than with wires, in the beautiful manner pointed out by Strehlke,* that is, by covering small drops of water with lycopodium, and placing these globules on the bars, when the directions of the vibrations will be shown by the motion of the particles of lycopodium."

In alluding to M. Hermann's results he notices:—†

That the results seem more likely to be effected by *a vibratory motion of molecules* than by a continuous effluence of caloric fluid. In fact, vibratory motion is the sole cause of the phenomena he observed, and not the heat produced by the friction.

In conclusion, he gives the following experiment:—

I prepared a considerable portion of styrole, and enclosed it in a long narrow glass tube, and caused it to vibrate by the friction of a moist cloth attached to a little mechanism set in motion by a spring for thirty hours, when nearly the whole of the styrole became solid; when the solid mass was again distilled, liquid styrole was obtained. The tube containing the substance unfortunately broke just as I was about to repeat the experiment, but I hope to be able to repeat it in a short time.

* Poggendorff's Annalen, vol. xi. p. 146. † Vide *Elec. Mag.* No. X. p. 140.

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CXXXIII. *Axial Electro-Magnetic Engines, the Axial Galvanometer, &c.* By Professor PAGE, M.D., &c. of Washington, U. S. From *Silliman's Journal* of July, 1845. Vol. xlix. No. 1.

(Abstract.)

PROFESSOR PAGE made his first attempt to obtain what he terms an axial engine, in 1838; but failed of success through want of suitable batteries: he has now overcome this difficulty by using Grove's battery, which at that time was not in existence. The axial engine is based upon the power of a coil conveying a current over an iron core. A suitable coil, connected with six pair of Grove's in good action, drew up within its centre a bar of iron or steel, weighing two or three pounds, and sustained it with its upper end projecting above the helix; and when the bar was very light as much of it projected above as below the helix. A bar thus circumstanced will not drop when the battery circuit is made and broken with great rapidity, but will exhibit a vibratory or dancing motion; if, under these circumstances a string be attached to the bar, the sensation to the person holding the string is like that felt by the angler when he has a bite.

The application of the above principle to the production of an electric-motive machine may be explained in a few words:—Two or more helices are secured upon a base-board with their axes in one and the same right line. Two soft iron cores are connected in a right line by a brass rod, and in such positions that when one has reached its position of equilibrium within the one coil, the other is being acted upon by the other coil; the motion is directed by a sliding frame, and is conveyed to a crank, which moves a fly wheel; the axis of the wheel forms an electrotome for inverting the current in the respective coils, and so continuing the oscillatory movement. It is obvious that the bars might be fixed and the helices be made to move, without departing from the principle; but, practically, this is not desirable. The bars may be hollow or solid, solid being best; and soft iron being better than steel. After steel bars have been used for a while, they become of course permanently magnetic; and, if the battery be now detached, and the machine be worked mechanically, it is converted into a magneto-electric machine, and will give the usual sparks and shocks.

"The peculiar advantages," says the author, "of this arrangement, are as follows; *First*, a continuous action may be maintained through a very great distance, as will be by-and-by explained. *Second*, the retardation common to all other

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forms of electro-magnetic engines cannot occur here; for, as the bars to be magnetized are small, they are very rapidly charged; and whatever magnetism they may retain after the galvanic current is intercepted in the helices, cannot retard their motion, as there can be no attraction between the copper wire of the helices and the enclosed iron bar. Hence, with a given quantity of battery surface, the maximum of speed and power is obtained. The retardation from the permanent retention of magnetism, the time occupied in charging a magnet to saturation, and the time required to discharge the magnet are serious obstacles in the way of obtaining any available power in ordinary electro-magnetic machines, and occasion that singular anomaly—that the active power of such machines diminishes as their rate of revolution increases. Add to these difficulties, the influence of secondary currents, which, as I have shown several years since,* magnetize a bar of iron after the battery current is cut off, and the *third* advantage of the new engine will be appreciated; for, in the first place, the secondary current occurs in all other forms of electro-magnetic machines when the armatures or magnets are very near the point of greatest action; but, in this engine, the secondary current occurs at the farthest possible distance from this point; and, in the second place, the secondary current has no perceptible influence upon the enclosed bar when it *does* occur."

"In addition to the power of the helix in drawing the bar within itself, I have availed myself," says the author, "of an extra source of attraction, viz. the actual power of the magnet, which receives an additional impulse by the attraction between it and an armature or bar of soft iron. This impulse, which is powerful, is received at an unfavourable moment, as it is nearly at the end of the stroke, when the crank is only a short distance from the dead point; but I have made use of it nevertheless to advantage, by an arrangement, which I will describe in the next number."

The double axial reciprocating engine is constructed with a U form bar, and a double set of coils. "A bar of iron of this form, having its legs 10 in. in length, was mounted upon a sliding frame; part of its weight counterpoised, and its legs inserted into two helices of 3 in. in length; the helices were then connected with the battery, and the bar was drawn through the helices, until it rested upon its bend. Thus, with a single pair of helices, and a single bar of iron, a continuous impulse was given through the space of 10 in., affording at

* Silliman's Journal, 1838, vol. xxxiv. p. 372.

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once the elements of the most simple and efficient exhibition of magnetic powers as a propelling agent."

Professor Page has constructed an engine of this sort with a 6-inch stroke, which is the most powerful engine of the kind that he has seen; and its operation is so encouraging that he is preparing another with a 12-inch stroke: he has also availed himself of the direct power of the magnet upon a bar of soft iron, and thus gains ten per cent. in power.

The axial galvanometer is closely akin to these machines. In reference to other galvanometers, the author says, very truly, "there is one liability to errors from a source not sufficiently regarded, viz., the frequent disturbance of the power of the needle, by the magnetizing power of the current in the coil, which, if the needle is in such a position as would naturally result from the action of the coil, will slowly increase its magnetic power, and if forced by accident or otherwise into a reverse position, will diminish its power, and ultimately reverse its polarity, provided the current be powerful, and the action continue for any length of time."

In the *axial galvanometer*, a U bar of soft iron is suspended from the short arm of a beam balance, so that its two ends or poles can enter within two helices placed beneath it, so that when a current passes through the helices, the bar is drawn down, and the force or actual weight is indicated by the balance; so that the instrument might almost be termed an *absolute* galvanometer. The helices are placed upon a shelf, which can be raised or lowered at pleasure, in order to alter the sensitiveness of the instrument; when the legs are just within the helix the action is slight, but when the box is well inserted, the maximum of effect is obtained. This instrument is not intended as a sensible galvanometer, but is calculated to measure the force of large batteries. "It affords at once a valuable instrument for determining the properties of helices of various sizes and lengths, of magnets, and of all those relations and proportions between battery surface, size of iron for magnets, and number of convolutions and size of wire, which must be determined before the economy of magnetic power can be settled."

The watchman in his box, is an illustration in *physique amusante* of these principles. A helix is mounted upon a stand, in a vertical position, and the connections are so made, that, when the current passes, the core starts up and exhibits the figure of a man, which disappears when the circuit is broken.

The galvanic or magnetic gun consists of four or more helices arranged consecutively. A bar moves freely through

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these helices; and, by means of a wire attached to the end near the breech, connection with the several helices is made and broken in succession: the iron acquires such velocity by the action of the four helices as to be projected to a distance of forty or fifty feet.

We must not omit to mention that, as the axial machines promise to be of practical value, Professor Page was desirous of securing them by patent, while he gave to the public the galvanometer, &c. But, as the following extract will show, he is peculiarly circumstanced:—"Situated as I am, being one of the examiners of the Patent Office, it becomes necessary for me to take an unusual step to secure my title to this novel invention. By the statute law, no officer in the Patent Office can take out a patent for any invention, nor acquire any interest in a patent after his appointment to office. Consequently, if one so situated should make an important invention, he can have no security as long as his invention is known to a second person. As publicity without abandonment is the strongest possible testimony in favour of an invention, I have resorted, with your indulgence, to the pages of this Journal, to establish the date and nature of my invention, and to give due caution to all as to its use in any way to engender liability. Although I cannot, without resigning my office, enjoy the usual right of invention, yet I have every reason to believe that Congress will, by special act, authorize me to receive and hold a patent for this and some other kindred inventions."

CXXXIV. *New Experiments on the Torpedo.* By M. Matteucci. "Comptes Rendus," Sept. 8, 1845.—Vol. xxi. p. 575, No. 9.—Letter to M. Blainville.

(Translation.)

The interest that you have always shown for my works on animal electricity have induced me to hope that the Academy will excuse my again coming to speak to them about the torpedo. I lately had the opportunity of having a certain number of living torpedos, and I did not fail to make a few experiments, of which I will give you an account. Although I have well established, by a great number of experiments, that the discharge of the torpedo never occurs except through a conducting arc, established between the back and the lower belly, I always see, and especially in some German works, that they speak of the possibility of having a discharge of the torpedo by simply touching it in any point of

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the back or belly with a conducting body, the fish being insulated. I have, therefore, again given my best endeavours to put the result to which I have arrived beyond all kind of doubt. I began by perfectly insulating the living torpedo, and I prepared a certain number of galvanoscopic frogs; I put several of them with their nerves on the back of the torpedo. I placed other galvanoscopic frogs upon a plate of glass perfectly varnished, and resting upon an insulating foot. The nerves of the frogs thus insulated touched the back of the torpedo. On irritating the torpedo, there was no difficulty in obtaining the discharge; the frogs lying on the torpedo contracted, whilst there was not the least movement in those that were insulated, and which, nevertheless, touched the torpedo with their nerves. It was only necessary to touch these frogs with the hand, or with any body communicating with the ground, in order to see these frogs contract instantly that the torpedo discharges. It is only when the nerves of the insulated frogs are extended over a large portion of the organ of the torpedo, that any contractions are seen in the insulated frogs. I have already shown that an electric current may be obtained by connecting two different points of the same surface of the organ with a conducting arc.

It is, therefore, plainly proved that the discharge of the torpedo always occurs through a conducting arc, which touches, at the same time, with its extremities, the vertical and dorsal face of the animal. I have again studied the direction and intensity of the electric current of the torpedo after having cut the organ normally to the prisms, of which it is composed. I cut the organ almost through half its thickness; the torpedo was perfectly insulated, and the upper part of the organ was sustained with hooks and silk cords. In this manner I could act with the extremities of the galvanometer sometimes upon the dorsal half, and at other times on the ventral half of the organ. The following are the results of the two experiments: the current of the dorsal part was, in one case 34°, in the other 35°, always directed from the back to the internal face, as usual; the current of the central portion was, in one case 3°, and in the other 4°, and directed as usual from the internal to the ventral face in the galvanometer. I left the two halves of the organ in contact, and I closed the circuit by again applying the two extremities of the galvanometer to the two faces of the fish; in this case, it was the extreme organ that gave the discharge. The intensity of the current was 40° in the first, and 45° in the second experiment. These three experiments were repeated upon the same torpedo, by operating as quickly as possible. The great intensity of

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the current of the dorsal portion, compared with that of the ventral portion, seems to me to be explicable by the greater number of nervous filaments that ramify in this portion. It follows from these experiments, that, although the organ may have been cut in half, the discharge that has been obtained, when the two portions touch each other, is just the same as that of the entire organ. This is an experiment that shows that each prism, composing the organ of the torpedo, is a multiplying apparatus, the different parts of which may act separately or together, in giving, in this case, stronger effects, which are almost the sum of the actions of the different parts. I thought it of some interest to compare the duration of the excitability of the motor nerves with that of the nerves belonging to the electrical organ. I have seen several torpedos, from which we had ceased to obtain muscular contractions on irritating the spinal marrow, again give the electric discharge by irritating the fourth lobe, or the nerves that go to the organ. The sensibility of the electric nerves is still better demonstrated when we act upon the organ of the torpedo, separated for some time from the animal.

This is an experiment that I have repeatedly made; and it is one which I still believe to be very important. On an organ of a torpedo, separated for some time from the animal, the galvanoscopic frogs are always seen to contract, when the little filaments that ramify in the organ are in any way irritated. On making this experiment carefully, the discharge is seen to be limited to very small parts of the organ, those in which the nervous filament that is irritated is seen to be lost. There is a very simple mode of making these experiments, which is to cover the surface of the organ with the greatest possible number of galvanoscopic frogs.

One of these frogs is then seen to contract; whilst another beside it does not move. In these experiments, it is also demonstrated that the nerves of the electric organ, like the motive nerves, have the property of losing their sensibility, beginning from the central parts and retiring towards the extremities. When, on irritating these nerves at the points nearest to the brain, no discharge is obtained; it may again be obtained if points of the same nerves are irritated which are nearer to their extremities.

I have not failed to repeat my old experiment, relative to the action of the electric current upon the nerves of the organs. I have no change to make from my first results; the electric current always excites the discharge, when it begins to pass being direct, and when it ceases, being inverse. It is the action of the electric current upon the nerves called

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mixed, and which differs from that which M. Longet and myself discovered when acting upon simple or motive nerves.

I must further remark, as being the only difference found between muscular contraction and the discharge of electric fishes, that potash applied upon the electric nerves does not give rise to the discharge. The following are some new facts that I have discovered lately, and which may one day serve to establish the physical theory of the organ of the torpedo.

Meanwhile, I cannot resist insisting upon the principal consequences to which I am led by the great number of experiments that I have for so long a time made upon the torpedo: 1st, The cellular of which the prisms of the organ of electric fish are composed, is in truth the elementary electric organ, which only requires for its operating the excitation of the nervous filament belonging to it, and the chemical integrity of the albuminous substance that it contains. 2nd, These prisms are physical apparatus, destined like magnets, batteries, electro-dynamic spirals to multiply the effect of the elementary parts of these organs. Hence all the importance of the remarks that I have for so long made of the position of the two poles of the electric organ in the torpedo and in the gymnotus. In the torpedo, the prisms of the organ have their bases supported upon the ventral and dorsal faces of the fish; and these two faces are also the electrical extremities of the organ. In the gymnotus, on the contrary, the extremities of the prisms are supported toward the tail and the head of the fish, and in like manner the head and tail are the electrical extremities. I have for a long time been desirous that a silurus should fall into the hands of a philosopher, in order that the direction of the current, compared with that of the prisms of the organ, might be well studied. 3rd, With regard to the constant direction of the discharge of electric fish, it seems to me that it may be conceived in the following terms: most assuredly the nerves of the electric organ, like all nerves endowed with a specific power, are fit to propagate currents of nervous force (whatever be the nature of this force) in one direction only. And this we also know to be the case with the sentient nerves, with the root of the spiral marrow, &c.

Whatever be the intimate relation between electricity and nervous force, it is certain that the function of the torpedo unveils to us the fact, perhaps a simple one, of the disengagement of electricity under certain conditions, determined by a nervous current.

Let us, if you please, call this simple fact, *Electro-nervous Induction:* it will always follow, according to all the analogies of physics, that the direction of the electric current, de-

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veloped by the induction of the nervous current, will always be in a constant direction determined in relation to the direction of the same nervous current. Now experiment proves to us that this direction in the force acting in the nerves, actually exists, since there are nerves simply motors, and others, sensors.

I will not extend the hypotheses further, having made to myself a rigorous law to follow the simple and sure mode of facts in this very obscure part of physics.—[A.]

CXXXV. *Supplementary Notes to a Paper entitled "Facts and Observations relative to the Production of Light from Voltaic Currents," &c.* Art. lxxxvi. p. 516, *Electrical Magazine.* By W. H. Weekes, Esq.

The many communications with which I have been, and still continue to be, favoured, as well as the various inquiries which reach me in relation to the subject above named, have induced me to the publication of a few additional remarks, and, beyond these, I beg to state that there is no information which my past experiments or my present knowledge will enable me to communicate.

In the first place, and as regards experiments 7 and 8, with the subterraneous battery of 36 pairs, alluded to in paragraph (21), I perceive that I have used the terms "a small, but exceedingly brilliant spark," and "a speck of *intensely vivid light.*" I very much regret that I did not adopt instead, the words *exceedingly minute*, both as regards the spark and the speck of light, for such language would have conveyed a more precise notion of the actual results, and, it seems, might have prevented some misconstruction. However, the truth is, that the only value I attached to the experiment rests on the simple fact that light could be produced at all from such a series of buried plates, and this having been obtained, it is, as I presume, evident that, from an increase of series and other suitable arrangements, results comparatively illimitable may be realised.

I would by no means venture to assert, or even to suppose, that the battery (21) employed by me, especially as relates to amount of surface in the respective metals, is to be regarded as the one best suited to such a purpose. On the other hand, I should incline to *a decrease of surface and an augmentation in the number* of metals, except in such cases as where expense constitutes a secondary or unimportant item, and then *amount*

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of series without diminishing surface would claim my principal attention. We cannot doubt that to increase both surface and number under certain limitations would be advantageous. Circumstances have generally rendered an eye to expenses a condition of the first importance in my own experiments on the production of voltaic light, otherwise I would long since have brought my "suggestions" fully to the test, which alone can decide their value or their insufficiency.

Neither the size of my plates nor their number was regulated by design, nor were either of these conditions even left to my choice; an opportunity occurred for procuring at a moderate cost the temporary use of the metals for the short space of about fifty hours, at the expiration of which, and when they had only been under ground rather more than eight hours during one singularly dark night, I was necessitated by my contract to take up the battery and restore the plates to their original destination.

Many hundred pairs of plates would, doubtless, be required in a subterraneous arrangement, in order to produce a permanent or occasional electric light possessing the character of utility, as regards the purposes of illumination.

I have had but one opportunity of repeating my experiment with a battery nearly the same in all respects as the one alluded to in page 524 of the *Electrical Magazine*, and in the second instance I did not obtain either the spark or light which occurred to me on the first occasion, for reasons which, as I believe, may be thus explained. Numerous trials in the shape of actual experiments with variously formed batteries, have proved to me beyond all question that the difference of electrolytic influence arising from different earths is much greater than from their appearance *à priori* we should be led to anticipate. The merely physical character of an earth is not sufficient to determine its properties as an excitant of the metals. Chemical constitution has, doubtless, much to do with the resulting electric currents, and I more than suspect there are yet other causes, both contingent and uniform, which operate most materially; and in all these particulars we have still much to learn, while, unfortunately, the key to this information can only be purchased by a long train of careful and expensive trials.

In my first experiment (21) the metals were sunk in a loamy soil—a deep red plastic clay of almost uniform moisture and consistence, and which, excepting the depth of a few inches on the surface, had evidently remained undisturbed for ages. The surfaces of the zinc plates were bright and regular, and those of the iron exhibited very little of the usual coating

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of carbonate and black protoxide of the metal, although, perhaps, the latter circumstance was of little consequence in the negative plates. The earth employed to bury the battery was reduced as fine as possible, and especial care was, moreover, taken to fill completely and evenly the interstices between all the plates, a slip of thin board being used lightly as a sort of rammer for the purpose. This experiment was made in a locality eight miles from my residence, and could be repeated in the same place. My second trial took place nearer home, and the earth in which the latter series of plates was deposited, as far as physical appearances are concerned, very much resembled that used in the first instance, the texture, however, being manifestly looser, owing, as I conceive, to the presence of less alumine. Neither *spark* nor *light*, as I have previously said, could be perceived this time, but most of the other experiments described at page 524 *Elect. Mag.* were as decidedly successful as formerly, as were also several which had newly suggested themselves to my mind. I ought to mention that the duration of my battery was again limited to a few hours, as on the first occasion, so that I had not time fairly to investigate any sources of failure, although I can scarcely doubt that such as I experienced arose principally from a difference of electrolytic power in the two coils.

I believe the best plan for a subterraneous battery is to arrange the plates in an oval, or rather, in a narrow horse-shoe form, so that the action of the metals may be continuous without the aid of connecting copper bands, from off which it is possible a part of the current may be lost by conduction into the vicinal earth. A *very stout* terminal wire or rod should proceed out of the ground from each end of the battery, and it will prove an advantage if these terminals ascend from the respective ends of the arrangement, each through a strong glass tube. I invariably consider, while engaged in these arrangements, that too much pains cannot be taken to insulate from the earth all but the plates themselves.

I regret to be obliged to add an expression of my fears, that such is the amount of expense incident to the requisite series of experiments on this interesting subject, that it has little chance of being perfected by an individual, or even by the union of several, however devoted to its accomplishment, unless they abound in riches, and these, alas, seldom fall to the possession of men ardently engaged in the pursuits of science.

Sandwich, 14th August, 1845.

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CXXXVI. *Experiments relative to the Heating of a Metal Conductor, that unites the Two Poles of a Battery.* Extract of a Letter from M. VAN BREDA to M. Arago.—"Comptes Rendus," Oct. 27, 1845. Vol. xxi. p. 961, No. 17.

(TRANSLATION.)

I HAVE just made an experiment that seems to me to have some relation to that which Paul Hermann lately communicated to the British Association.

Having been occupied for several months in researches upon the vibratory movements obtained by passing a discontinuous electric current through metal wires, I was desirous of knowing, among other things, whether the molecular movement, produced in a bar of soft iron placed in the interior of a helix, through which a discontinuous current is passed, does not develope a certain degree of heat.

I directed a tolerably energetic galvanic current through a metal helix, wound round a wooden bobbin; in the interior of this bobbin I fixed a tube of soft iron, hermetically sealed at both ends; and through one of these ends I passed a thermometric tube open at both ends, and containing a bubble of coloured liquor. The metal tube contained a thermo-electric element of bismuth and antimony, the conducting wire of which passed out at the other end, and communicated with a very sensible galvanometer.

The least development of caloric in the substance of the metal tube would thus be communicated to the air contained in its interior, and produce an effect on the coloured bubble, and on the thermo-galvanic element, as well as on the galvanometer.

I first directed the current for a sufficiently long time through the helix that I might be convinced that the heat developed in the wire of the helix was not communicated in a sensible manner through the bobbin to the soft iron tube. The liquid bubble, as well as the galvanometer, remained motionless.

After the helix was quite cooled, I rendered the same current *discontinuous* by a rheotome that opened and closed the circuit about thirty times per second. The thermometric bubble, after a few seconds, began to move and to recede from the metal tube. This movement was sometimes so strong that it was thrust out of the tube; the galvanometer, at the same time, indicated that this movement did not depend upon a change of volume in the iron tube itself, but that it was really due to radiation from the internal sides of the

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heated tube by the special and repeated action of the currents at the moment when they rise and when they are interrupted.

The sound is heard as usual.—[A.]

CXXXVII. *On a New Method of Curing Certain Aneurisms, without Operation, by means of Galvano-Puncture.* By M. Jo. PETREQUIN, Chief Surgeon of the Hotel Dieu of Lyons. (Extract.)—From "Comptes Rendus," Nov. 3, 1845. Vol. xxi. p. 992, No. 18.

(TRANSLATION.)

THE cure of aneurisms without a bloody operation would certainly be a great conquest. They are, with justice, ranked among the most serious evils of the nosologic list. Indeed, if they are abandoned to themselves, death, with a few rare exceptions, is their habitual termination; if the surgeon undertakes their cure, the most frightful accidents may accompany his attempts, and not only do these attempts fail to cure, but in more cases than one it has happened that they occasion or accelerate the death of the patient.

The application of electricity had been proposed, but this project met with no results; and all that science has found in this respect is summed up in the following lines, written by MM. Marjolin and Benard, in 1833. "The idea has been entertained of exciting the coagulation of blood in the sac, by means of electricity transmitted to it, by needles plunged into the tumour; *this idea*, which is due to M. Pravas, *has not yet*, as far as we know, *been put into practice*." (Dict. in 25 vols. Art. *Aneurisms*, vol. iii. p. 50.) To assure ourselves of the fact, we communicated with M. Pravas himself, who told us that, in point of fact, it was a speculative view,—an induction with which his researches on hydrophobia had inspired him, but that the experiment had never been made with aneurisms, neither upon animals nor upon man.

It was a way, therefore, altogether abandoned, and a means entirely forgotten. However, before searching out another, I wished to know what dependence could be placed upon it; and to assure myself whether it were really valueless, I at first experimented, not upon animals whose blood fluids have very different properties, but upon human blood at the moment it was drawn from the vein by bleeding. The experiment gave me much hope; I thought it was a new question to be solved, and I immediately set to work. To make known my results, I shall divide the aneurisms into two catagories.

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Traumatic Aneurism of the Temporal Artery, cured at a single sitting by Galvano-Puncture.—On Aug. 4, 1845, Mr. Dasnyard, aged 19 years, a locksmith, at Lyons, was brought senseless to the hospital; he had just fallen from a second floor. There was a violent contusion of the left eye, and a fracture of the lower jaw on the median line. Toward the end of the treatment he was attacked with smallpox, which went through its period regularly. On September 9, I was able to direct my special attention toward a small tumour of the left temple that had attracted my observation. It was a traumatic aneurism of the temporal artery, of the size of an almond, of a soft consistence, and slightly sensible to the pressure of the finger. The tumour was on the course of the temporal artery, which might be followed to its surface; it was the seat of throbbings, isochronous with those of the pulse, which were visible through the skin, and which ceased, under the influence of a strong pressure below, to reappear again as soon as the pressure of the finger was removed. There was no doubt about the nature of the hurt. It was proved that this aneurism was due to the concussion of the artery. It occurred in consequence of the fall.

On September 10, I held a sitting of galvano-puncture, in the presence of several physicians and a great many pupils. I took two steel pins that were fine and sharp, and I thrust them in so as to cross them at right angles in the tumour, into which they penetrated about 2 c.m. I connected their heads with the poles of a battery. At the first contact there was an electric shock and a sharp pain; and these symptoms continued to increase in proportion as I augmented the dose of galvanism; their intensity became very great at the fiftieth pair, and I suspended the sitting; the duration of the operation properly so called, namely, the real action of the galvano-puncture, was about 10 or 12 minutes. I had thrice changed the direction of the galvanic current during this space of time.

During the manipulation I felt the pulsations progressively diminish; but, for fear of being deceived, I took care to have the phenomenon established by Drs. Gerin and Rambaud, who assisted in the experiment; the fact was real. It was not without profound satisfaction that I recognised that the throbbing had entirely ceased at the end of the sitting. The aneurism with isochronous pulsations was replaced by a solid and indurated tumour. The problem was resolved; I removed the pins; and the dressing consisted of bran and water retained with bandages.

The invalid, who had been excited, was pale and a little low spirited; he rose, and returned alone to his bed. Two hours afterward he was without suffering, and in the afternoon

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he ate as usual. (The observation was carefully renewed daily by M. Baumers, house surgeon.) No accident followed.

On the 12th, we examined the patient very attentively. The tumour no longer existed; not the slightest pulsation was felt; the temporal artery was in like manner obliterated above, for no throbbings were found, whilst they were very sensible in the points of its course inferior to the aneurism.

He was examined anew every other day, until his departure from the hospital, which took place on September 20. The lump which succeeded the tumour was gradually absorbed. This result was in like manner attested by M. Boucharcourt. The cure was completed.

The patient came to me a week afterwards; there was no deception about the cure.

Aneurism of the Ophthalmic Artery, and of the Origin of its branches with the Exophthalmic; Galvano-puncture.—A young man of the neighbourhood of Geneva fell on his face; and, about three months afterwards, he perceived a tumour in the socket of the eye, which presented all the characters of an aneurism; pulsations isochronous with the pulse; rustling noise, &c. The evil continuing to grow worse, I made a ligature of the primitive carotid artery, which had a momentary appearance of success. The throbbings appeared again after the second week; I then applied galvano-puncture; but this experiment presented the great inconvenience of being the first of my attempts in chronological order; and he had a fit, which was but of short duration. Notwithstanding the care that I had taken, the operative process left much to be desired.

Aneurism of the Elbow Joint, in consequence of Bleeding; Galvano-puncture.—An adult consulted me in the environs of Rive-de-Gier, for a very voluminous aneurism of the right brachial artery, the result of a bleeding badly managed. I made a sitting of galvano-puncture, which rendered the tumour more compact; to favour the complete coagulation, I applied continuous compression on the arm, and I also allowed the two needles to remain in the tumour; but he was very soon wearied of the compression, which I was, therefore, obliged to remove; simple acupuncture was insufficient, and he obstinately refused a second galvanic sitting, which might perhaps have succeeded. Spiritless and pusillanimous, he had not the courage to undergo a new proof, notwithstanding all we diligently said to him.

We shall now see what the different conditions of the problem are to triumph over the obstacles that aneurismatic sacs of a certain volume present.

Of the Operative Process.—The first indication is to modify the circulation in the different vessels, without which the

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column of blood will get the better of the clot as fast as it is formed. However, in the first observation, in which I desired to assure myself of the possibility of the fact itself, I did not apply any compression upon the artery, and I succeeded; but this precaution is indispensable when an arterial channel of larger size is concerned; it would even be useful to intercept by compression all the neighbouring communications.

In the sac, the blood should be stagnant and motionless, as nearly as possible; the patient should be lying down, or seated firmly in an arm-chair. In my open experiments upon blood, I have seen the clot form round the pins, and only when they were in contact; the phenomenon was active when they were placed across; it follows, therefore, that it is proper to imitate this arrangement, and that the sources of the clot must be multiplied when the aneurismatic sac is very voluminous, so that the clots formed in different directions, and in many points, easily finish by confounding themselves into a common coagulum. After the operation, ice may be applied on the centre of action, and the compression, if possible, may be continued around.

I employed steel acupuncture needles: the direct experiment upon free blood showed me that the one which corresponded to one pole, in order that it may act well, must cross with the needle corresponding to the other pole; and that, in voluminous aneurismatic sacs, they must be multiplied, in order to produce at the outset a good number of clots, so as to offer a sufficient nucleus for the common coagulum. There was a double difficulty; in penetrating to a clot of blood, the needles burn the skin, irritate the nerves, cause sharp pain to no purpose, and produce a terrible loss of electricity; hence an unfavourable inflammatory reaction, and the want of success; it is, therefore, necessary to insulate them in the tissues; I succeeded, by means of a layer of varnish, to the extent of 1 or 2 c.m. according to the thickness of the soft parts to be traversed.

To reconduct at pleasure the copper wires representing the poles, they are held in the hand. I insulated them by enveloping them, as well as the ends of my fingers, in a piece of silk. To produce a better coagulation of the blood, it is best to begin gradually with a certain number of pairs, and to proceed by rapid progression if the first effects are incomplete. The direction of the galvanic current may be repeatedly changed with advantage. The liquid of the battery must not be made too strong with nitric and hydrochloric acids, it eats into the metals, and the electric action soon becomes languishing; it must also not be too weak, otherwise the sanguine fluid would not coagulate.

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With these rules I have been able to realize with success the first application that has yet been made upon man, of galvano-puncture for the cure of aneurisms.—[A.]

Note.—At the sitting of the *French Academy*, on Dec. 8th, "Mr. Phillips, of London, addressed a note, the object of which was to prove that, for curing certain *aneurismal tumours* by means of galvano-puncture, he had made experiments that are several years anterior to those of M. Petrequin." This note was referred by the Academy to the same committee who had received the former communication.

SCIENTIFIC MEETINGS.

ROYAL SOCIETY.

Nov. 27th.—*On the Magnetization of Light and the Illumination of Magnetic Lines of Forces.* By Professor Faraday, D.C.L., F.R.S., &c.

"For a long time past the author had felt a strong persuasion, derived from philosophical considerations, that among the several powers of nature which, in their various forms of operations on matter, produce different classes of effects, there exists an intimate relation; that they are connected by a common origin, have a reciprocal dependence on one another, and are capable, under certain conditions, of being converted the one into the other. Already have electricity and magnetism afforded evidence of this mutual convertibility; and, in extending his views to a wider sphere, the author became convinced that these powers must have relations with light also. Until lately, his endeavours to detect these relations were unsuccessful; but at length, on instituting a more searching interrogation of nature, he arrived at the discovery recorded in his paper, namely, that a ray of light may be electrified and magnetized, and that lines of magnetic force may be rendered luminous.

"The fundamental experiment revealing this new and important fact, which establishes a link of connexion between the great departments of nature, is the following:—A ray of light issuing from an argand lamp is first polarized in the horizontal plane by reflection from a glass mirror, and then made to pass for a certain space through glass composed of silicated borate of lead, on its emergence from which it is viewed through a Nichol's eyepiece, capable of revolving on a horizontal axis, so as to interrupt the ray, or allow it to be transmitted alternately, in the different phases of its revolution. The glass through which the ray passes, and which the author terms the dimagnetic, is placed between the two poles of a powerful electro-magnet, arranged in such a position as that the line of magnetic forces resulting from their combined action shall coincide with, or differ but little from, the course of the ray in its passage through the glass. It was then found that if the eyepiece had been so turned as to render the ray invisible to the observer

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looking through the eyepiece before the electric current had been established, it becomes visible whenever, by the completion of the circuit, the magnetic force is in operation; but instantly becomes again invisible on the cessation of that force by the interruption of the circuit. Further investigation showed that the magnetic action caused the plane of polarization of the polarized ray to rotate; for the ray was again rendered visible by turning the eyepiece to a certain extent; and that the direction of the rotation impressed upon the ray when the magnetic influence was issuing from the south pole, and proceeding in the same direction as the polarized ray, was right-handed, or similar to that of the motion of the hands of a watch, as estimated by an observer at the eyepiece. The direction in which the rotation takes place will, of course, be reversed by reversing either the course of the ray or the poles of the magnet. Hence it follows that the polarized ray is made to rotate in the same direction as the currents of positive electricity are circulating, both in the helices composing the electro-magnet, and also as the hypothetical currents which, according to Ampère's theory, circulate in the substance of a steel magnet. The rotatory action was found to be always directly proportional to the intensity of the magnetic force, but not to that of the electric current; and also to be proportional to the length of that portion of the ray which receives the influence. The interposition of substances which occasion no disturbance of the magnetic forces produced no change in these effects. Magnets consisting only of electric helices acted with less power than when armed with iron, and in which magnetic action was consequently more strongly developed.

"The author pursues the inquiry by varying in a great number of ways the circumstances in which this newly-discovered influence is exerted; and finds that the modifications thus introduced in the results are all explicable by reference to the general law above stated. Thus the effect is produced, though in a less degree, when the polarized ray is subjected to the action of an ordinary magnet, instead of one that derives its power from a voltaic current; and it is also weaker when a single pole only is employed. It is, on the other hand, increased by the addition of a hollow cylinder of iron, placed within the helix, the polarized ray traversing its axis being then acted upon with great energy. Helices act with equal power in any part of the cylindric space which they enclose. The heavy glass used in these experiments was found to possess in itself no specific magneto-inductive action.

"Different media differ extremely in the degree in which they are capable of exerting the rotatory power over a polarized ray of light. It is a power which has no apparent relation to the other physical properties, whether chemical or mechanical, of these bodies. Yet, however it may differ in its degree, it is always the same in kind; the rotation it effects is invariably in one direction; dependent, however, on the direction of the ray and of the magnetic force. In this respect it differs essentially from the rotatory power naturally possessed by many bodies, such as quartz, sugar, oil of

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turpentine, &c., which exhibit the phenomena of circular polarization; for in some of those the rotation takes place to the right, and in others to the left. When, therefore, such substances are employed as dimagnetics, the natural and the superinduced powers tend to produce either the same or opposite rotations; and the resulting effects are modified according as they are cumulative in the former case, and differential in the latter.

"In the concluding section of the paper the author enters into general considerations on the nature of the newly-discovered power of electricity and magnetism over light, and remarks that all these powers possess in common a duality of character which constitutes them a peculiar class, and affords an opening, which before was wanting, for the application of these powers to the investigation of this and other radiant agencies. The phenomena thus brought to light confirm the views entertained by the author relative to the constitution of matter as being spheres of power, for the operation of which the conception of a solid nucleus is not necessary; and leads to the presumption that the influence of magnetism on bodies which exhibit no magnetic properties consists in producing in them a state of electric tension tending to a current; while on iron, nickel, and other bodies susceptible of magnetism, currents are actually established by the same influence."

CHEMICAL SOCIETY.

May 19.—*Experiments on Ozone.* Mr. WILLIAMSON.—This name, given by Schöenbein first to the peculiar odour of the oxygen of electrolysis, was described as having given rise to many speculations; among which was that which gave it out as a new elementary body, derived from a presumed decomposition of nitrogen;* which opinion, however, had been lately disproved by Marignac,† who showed that the odour was produced in the electrolysis of water free from nitrogen. Mr. Williamson's experiments go to prove that ozone is a compound body, and that one of its elements is hydrogen; for, having excluded the last element, from any other source, by obtaining the oxygen gas with ozone from the decomposition of a salt of copper, a process in which no hydrogen is generated, and passing the oxygen over metallic copper, which had been reduced by carbonic oxide gas, a sensible formation of water always resulted. The bleaching power of ozone shows it to be a peroxide; and it must, therefore, be a higher oxide of hydrogen than water, although not the peroxide of hydrogen of Thénard, which is not volatile, like ozone, but inodorous and fixed. Mr. Williamson finds also that the substance produced by the action of phosphorus on air is different from ozone; and that its effect, observed by Schöenbein, in decomposing iodide of potassium and liberating iodine, is the result of the joint action of phosphoric acid and free oxygen upon a solution of that salt.

* Vide *Elec. Mag.* Vol. i. p. 472.
† *Vide* present No. p. 33, and elsewhere.

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BRITISH ASSOCIATION, 1845.

(*Continued from p.* 142.)

Saturday, June 21.—MATHEMATICAL AND PHYSICAL SCIENCE.—Sir JOHN HERSCHEL laid before the section the *Report of the Magnetical and Meteorological Committee.* As this report had been previously circulated among members, Sir John did not think it necessary to occupy time in reading it.

On the Results of the Magnetic and Meteorological Observations at Sir T. Brisbane's Observatory, in the year 1842. By J. A. BROWN.

From a comparison of five months in 1841 with the corresponding five months of 1842, the yearly movement of the north end of the declination magnet is about five minutes towards the east. The horizontal component of the earth's magnetic intensity increases, and the vertical component diminishes considerably in the year; the diminution of magnetic dip being about five minutes. A new method has been adopted in order to obtain the temperature corrections for the bifilar and balance magnet; it is described in the sixteenth volume of the Transactions of the Royal Society of Edinburgh. It was merely mentioned that very consistent results had been obtained by different methods of comparison of the usual observations for the positions and temperatures of the magnets. When the observations of the balance magnetometer are corrected by this method, the diurnal range of the vertical intensity has been found, like that for the horizontal intensity and declination, to increase regularly from the winter months to the summer months. The annual period of the horizontal intensity, as deduced from the corrected observations of the bifilar magnetometer for 1842, is striking; a minimum of intensity occurs before or about each equinox, and a maximum before or about each solstice. The observations at Toronto, in Canada, in 1842, when corrected by the same method indicate exactly the same periods. The monthly means for Makerstown and Toronto were projected in curves, which were exhibited; the two curves were almost identical, the increase of horizontal intensity being greatest in the end of the year at Makerstown. The corrected observations of the balance magnetometers confirm, in some sense, the results from the bifilar, inasmuch as they also show the same annual periods of maxima and minima for the vertical intensity. As a severe test of the accuracy of the instruments and the methods adopted, the results for the magnetic dip deduced from the true force magnetometers were compared, both as to diurnal and yearly change of dip, with the results obtained from the inclinometer, and they were found to agree.

On the Connection between Magnetic Variation with certain Peculiarities of the Earth's Structure. By J. M. SAXBY.—The author thinks that there is an angular coincidence between the direction of mountain ridges and the local curve of equal magnetic variation, of from 65° to 70°. He thinks it might be interesting to notice the position of mineral veins in mountainous districts.—In reference to the above remark, Professor Forbes could not assent to the views proposed by Mr. Saxby, and said that the most ordinary acquaintance with the effect of mountain chains would be sufficient to show their incorrectness.

CHEMISTRY AND MINERALOGY.—*On Recent Experiments with the Gas Voltaic Battery.* By Professor GROVE.—We described the arrangement of this battery in our last number (p. 158). Professor Grove, in the present paper, pointed out how perfectly

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endiometric analysis might be carried on with the gas battery, provided some attention were paid to a few sources of error.

Some experiments on the combination of phosphorus and sulphur with oxygen in the battery were then named; and it was found that any body capable of volatilization gave a galvanic action with oxygen in the other tube. Camphor, alcohol, ether, and other bodies proved the constancy of this effect. It was then stated that, in all cases, it had been found that chemical action and voltaic action were convertible into each other.

Recent Experiments on Ozone. By Professor SCHÖENBEIN.

Professor Schöenbein was first induced to undertake his researches from the obscurity which rested upon the phenomena of the odour produced during the galvanic decomposition of water, resembling the smell of an electrical machine, and during thunder-storms. In pursuing these researches, the author was led to the discovery that the smell was always developed at the positive pole; that it was capable of being preserved in closed bottles; that heat destroyed it; and that many of the metals had also the same power. Experiments were made with a view to discover some means of procuring ozone easily; and, after the trial of a great many bodies, it was found that phosphorus was particularly suited for the purpose. If a piece of phosphorus is placed in a bottle of common air, when maintained in a moist state, it readily produces this peculiar principle, on which the electrical smell depends. Several experiments were shown to illustrate the effects of ozone in bleaching litmus paper, and paper coloured with indigo, or a solution of that substance. If powdered iron or silver are placed in vessels containing ozone, the smell is immediately removed, and the metals exhibit a kind of oxidation. Other bodies were named as producing a similar result, and many chemical decompositions were found to arise from exposure to the action of this peculiar principle. Solution of iodide of potassium is rapidly decomposed, iodine being set free; this was shown by a mixture of this salt with starch, which, on being exposed to the action of ozone, turned blue by the formation of iodide of starch. Bromide of potassium was also decomposed by this principle, and bromine liberated. Salts of iron were shown to undergo the same changes. A number of organic bodies, both vegetable and animal, destroy the ozonous smell. If ozonized air be made to pass through a tube, and this tube gently heated, all the properties of ozone are destroyed. This was shown by experiment. By the inspiration of ozone, similar effects are produced on the lungs to those resulting from chlorine and bromine. A mouse was killed in five minutes; and the experimentalist himself was seriously affected by breathing an atmosphere charged with this odour. By electrolysis, gold or platina points are necessary for the development of ozone. The electrical brush, in all cases, produces the same effects as those above described; all the decompositions can be produced, and the same smell is distinctly evident. In this case, as in the other, heat and some of the metals also destroy the odour. Endeavours have been made to procure ozone in an isolated state, but they have not been successful. Ozone, although at first supposed to be an elementary body, was afterwards considered as a compound of oxygen and hydrogen. The fact of heat destroying this peculiar odour at once, shows that this principle is produced from the elimination of an oxygen composed from the decomposition of water. This is quite in accordance with the views entertained by Marinac, who has pursued the investigation with great industry, and who has published a memoir, in which his views are luminously set forth. The author of this report is of opinion that ozone will turn out to be a compound isomeric with the binoxide of water. A theoretical view of the production of this body was then entered into. The ordinary action of phosphorus undergoing oxidation in the atmospheric air was explained, and the remarkable fact stated, that, although phosphorus was luminous in moist air, it was not so in perfectly dry air. Ozone may now, therefore, although long regarded by Professor Schöenbein as an elementary body, be looked upon as, in all probability, a tritoxide of hydrogen. Its bleaching properties are very remarkable, and it may possibly be of considerable practical utility.

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Monday, June 23.—MATHEMATICAL AND PHYSICAL SCIENCE. —*Remarks on the Periodiocity of Magnetic Disturbance.* By the Rev. H. LLOYD.

When we examine, for the first time, the chart of the changes of one of the magnetic elements during a day of disturbance, we do not hesitate to pronounce that the causes which produce these changes, so apparently capricious, belong to the class which, from our ignorance of the laws, we are accustomed to denominate "accidental" or "irregular." Experience, however, has shown that these phenomena, and therefore also the forces which produce them, are subject to laws which require multiplied observation alone for their development. A few months of systematic observation is sufficient to show that these apparently abnormal movements of the magnet recur more frequently at certain hours of the day than at others. Professor Kreil seems to have been the first to notify this remarkable fact. In a letter addressed to M. Kupffer, dated in January 1839, he observes, that all hours of the day do not appear to be equally favourable to the development of this phenomenon, "that disturbances begin" much more frequently in the evening than in the morning hours, and "hardly ever begin in the latter part of the forenoon." In a letter addressed to Colonel Sabine, dated in July 1840, Professor Kreil has entered more minutely into the question, with the light of the observations of an additional year. He then observes that "the *least* disturbance takes place in the declination from 8 to 10 a.m., and the *greatest* from 8 to 10 p.m.;" that "the declination is *increased* by the disturbances of the forenoon and the middle of the day, and *diminished* by those occurring in the evening hours;" that the effect of disturbance upon the horizontal intensity is, in general, a *diminution* of that element, this diminution being, however, more considerable "during the hours of the night and morning than in the forenoon and afternoon." Professor Sabine has lately discussed this matter more elaborately, by a mode, for the most part, the same as that of Professor Kreil, namely, to *separate* the individual results, which differ from the monthly mean, corresponding to the same hour, by a quantity exceeding a certain arbitrary limit; to treat them as the effects of perturbing causes, and to examine the *frequency of their occurrence* at the several hours of regular observation. By this mode of examination, Colonel Sabine has been led to the result—a result partly agreeing with, and partly differing from, that deduced by Professor Kreil—that "the causes which produce *easterly* deflections have their maximum frequency of effect at ten hours, and those which occasion the *westerly* deflections at twenty hours. The minimum of both occurs nearly at the same hours, namely about two or four hours." Analogous conclusions are deduced from the disturbance of horizontal intensity, which are on the whole subtractive, and have their minimum at four p.m., the time of maximum intensity; their maximum, on the other hand, occurs about the time of their nocturnal minimum of intensity, or from ten to sixteen hours. Colonel Sabine compares the monthly mean of the several hours, as deduced from the *whole* body of observations, and as deduced from the *remaining* observations when the excessive deflections are laid aside. Of the propriety of this Dr. Lloyd would not now speak, as his remarks would be irrelevant. With regard to an annual period, Professor Kreil and Colonel Sabine had arrived at different conclusions; according to Professor Kreil, "the perturbations are much more frequent in the winter than in the summer months," and that not merely because the cause which produces the regular diurnal changes is then more feeble, but also because (according to Professor Kreil) the disturbing forces are then actually of greater intensity. According to Colonel Sabine, "the disturbances (of declination) appear to be distributed throughout the year, without any marked inequality as to number or direction," except that their number appears to preponderate somewhat in the month of October. With respect to the horizontal intensity, Colonel Sabine appears to agree with Professor Kreil, that the number of disturbances are greater in winter than in summer months. Dr. Lloyd now laid before the Society the results to which he had himself arrived by a different mode of investigation. The problem he proposed, was to determine the *law of probability* of disturbances, as dependent upon the hour of the day and the season of the year, a question of great

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importance in any physical theory. Other methods take no account except of disturbances exceeding a certain arbitrary limit. The deduction of the law, although laborious, is simple in principle. We have only to take differences between each partial result, and the monthly mean corresponding to the same hour, and to combine these in the same manner as the errors of observations are combined in the calculus of probabilities. Thus, the square root of the mean of the sum of the squares of these differences is a quantity analogous to the *mean error* in the partial observations of a constant quantity; and the *probable* disturbance at any hour is inferred from this, by multiplying it by a constant factor. Dr. Lloyd then gives the mean *hourly* and the mean daily disturbances as thus deduced, and he shews that the tendency to disturbance observes a *regular period* both in magnitude and direction, connected with the diurnal movement. From the little change during the night, Dr. Lloyd was led to consider the magnet at night in its *normal* position, from which it was made to deviate during the day by the influence of the sun. In this point of view, the regular diurnal progression may be described as a westerly deviation of the north end of the magnet, commencing about an hour after sunrise, reaching its maximum a little after 1 p.m., and then diminishing until a few hours after sunset, when the magnet returns nearly to its normal position. The *mean* disturbance observes a period almost the reverse of this, both in magnitude and direction, its value being nearly constant during the day, whilst it is largely developed during the night in a direction opposed to that of the *regular* day movement. Several results are given, from which it appears that the mean disturbance preserved an *annual*, as well as a *diurnal*, with a *maximum* in summer and *minimum* in winter. Dr. Lloyd concluded by stating that he had commenced a series of observations, in order to distinguish the two kinds of disturbances by their external character, and to resolve the complex resultant into its simpler elements.

On the Elementary Laws of Statical Electricity. By W. Thompson, B.A.—The principal object of this paper was to show that almost all the results adduced in memoirs by Mr. Snow Harris and Prof. Faraday, which refer to electricity in equilibrio, are necessary consequences of the mathematical theory, and that none are at variance with it.

Tuesday, June 24.—Mathematical and Physical Science.—*Abstract of Observations at the Kew Observatory.* By Mr. F. Ronalds.—In the abstract, Mr. Ronalds confined himself to an enumeration of the different heads of observation, and particularly of the apparatus employed.

Monday, June 23.—Chemistry and Mineralogy.—*On the Mechanical Equivalent of Heat.* James P. Joule.—The author gave the results of some new experiments, in order to confirm the views he had already derived from experiments on the heat evolved by magneto-electricity, and from experiments on the changes of temperature produced by the condensation and rarefaction of elastic fluids.

On the presence of Ozone in the Atmosphere. Prof. Schöenbein.

The action of heated platina upon the vapour of ether was shown, and the production of that principle, to which the name ozone has been given, was manifested by the bleaching of litmus paper, and the decomposition of iodide of potassium. The production of the same principle during all the manifestations of phosphorescence, was brought forward as a proof of the existence of ozone in the atmosphere. Ozone was found to be of a deleterious nature, but it was removed most readily from the atmosphere by the organic matter, to the action of which it was constantly exposed. Prof. Schöenbein then went into extensive specula-

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tions on the phosphorescence of the sea, which he regarded as a phenomenon in all respects resembling the slow combustion of phosphorus; and this, with the luminous phenomena exhibited by insects, by rotten wood, and by putrefying matter, he attributed to the oxidation induced by the influence of ozone, which was now most distinctly proved to be a peroxide of hydrogen.

Prof. Faraday begged to correct an error into which he had been led some years since, when he stated this peculiar odour to be due to the formation of nitric acid in the air. Although he had formed nitrate of potash by the action of electrolyzed air or caustic potash, and consequently shown that nitric acid was formed, he believed the odour to depend upon some other cause. He could not go with Prof. Schöenbein in all his speculations, but he thought the inquiry of importance. Prof. Schöenbein stated that during a recent snow-storm at Basle, iodide of potassium was decomposed by the action of the atmosphere in about six minutes.

Description of a Colossal Hydro-Electrical Machine. By W. G. Armstrong, Esq. See page 161 of the present number of the *Electrical Magazine*.

Medical Science.—*On Endosmose and Exosmose, as influenced by Galvanism.* By Dr. Leeson.

He first adverted to the experiments of Dutrochet and Porret; from errors in these experiments, the original idea of Dutrochet was soon abandoned, viz., that electricity was the efficient cause of the phenomena; these errors were explained. The author investigated the subject by a series of experiments free from those errors, some of which were exhibited: from which he concluded that endosmose and exosmose were greatly promoted by electricity, but that although this is so, the phenomena may exist without any galvanic or electrical condition being present.

Wednesday, June 25.—Mathematical and Physical Science. —*On the Origin of the Aurora Borealis.* Rev. G. Fisher.

The author professes to establish the following proposition:—'The principal displays of the aurora occur near the edge, or exterior limits of the frozen sea, where the process of congelation goes on with the greatest rapidity. The aurora is an electrical phenomenon, and arises from the positive electricity developed by the congelation of humid vapours, and the consequent induced negative electricity of the surrounding portions of dry atmosphere. It is the accompanying indication of the restoration of the electrical equilibrium, which is effected by the intervention and conducting power of minute frozen particles, which particles are rendered luminous by the transmission of the electricity, and thereby give rise to the phenomenon of the aurora.'

Tuesday, June 24.—Zoology and Botany.—*On the Influence of Galvanic Electricity on the Germination of Seeds.* By Prof. E. Solly.—An account of those experiments will be found in page 159 of the last Number of the *Electrical Magazine*.

Wednesday, June 25.—Capt. L. L. B. Ibbetson exhibited a collection of electrotyped plants. Most of the specimens belong to the family Orchidaceæ; but there were many specimens of other plants, and some fungi, with their forms beautifully preserved. We believe these specimens are at the present moment in the Museum of Economic Geology.

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FRENCH ACADEMY.

July 7.—*A Poplar-Tree struck by Lightning;* described by M. RENAULDIN, a physician, in a note to M. Arago.

It appears that scarcely a quarter of a second elapsed between the flash and the report; that the highest tree was struck; that a score of fragments were detached, three were 2 yards long, eight were 1 yard, and the rest like large matches; they were transported to a distance of 20 paces, and the leaves were untouched; that about a yard from the ground, and opposite to the great rent, but a little lower, a piece of bark a yard long and 4 in. wide, was detached from the wood, and held on by its extremities; that the gardener, who was in the green-house was knocked down; on recovering, after 5 minutes, he declared that "*he had neither seen nor heard anything;*"—that the house near which this occurred, had no lightning-conductor. M. Renauldin examined the route taken by the lightning, in passing from the tree to the green-house, which was about 5 yards distant, and with the door closed. He says:—

"I perceived on the wall, which is near to the tree, several rows of iron wires intended for supporting the espalier plants. These wires, which are rather larger than a knitting-needle, are arranged some horizontally, others perpendicularly, with spaces so as to form squares of 20 c.m. (8 in.) The knot existing at each point of junction of the wires, is slightly oxydized, and has left a black impression of 2 or 3 c.m. on the white wall, so that the latter presents as many spots as there are points of junction in the wires. Following the wires that have a horizontal direction, I observed one which, on arriving at the angle formed by the wall of the garden and that of the green-house, curved in order to pass over the door of the latter, and enter in by a small opening formed between two panes of glass. The lightning evidently followed this wire; and entering into the green-house it starred and pierced one of the panes of glass, and dispersed the neighbouring pane in fragments; it then knocked down the gardener who was in its route; it again followed the direction of several other iron wires applied to the wall; and, after having traversed the space of above three yards, it was dissipated at one of the angles of the green-house, making a round hole in the earth, funnel-shaped, the exposed part of which was about 20 c. m. (8 in.) across, and the depth apparently about the same. All the iron wires when examined attentively, presented no signs of torsion, nor of solution of continuity. Their points of junction, within the green-house, were, like those without, slightly oxidized; and left black spots upon the wall, as if a few grains of gunpowder had been burned on these places. The fragments of wood that were torn from the tree, were not in a direction between the tree and the green-house; they were thrown by the violence of the lightning far away to the left."

July 14.—M. DESBORDEAUX gives some "*New Observations* *

* Vide *Elec. Mag.* Vol. II., pp. 55, 56.

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relative to Plating upon Steel," in which he states that, for rendering the adhesion complete, iron conductors are preferable to copper, lead much more so; and that mercury would be still better were it not that it interferes with the simplicity of the apparatus.

M. Ducros submits to the Academy a Memoir entitled:—"*Experimental studies of practical medicine, or the jet of blood in bleeding, which is instantly increased in man, and in animals, by electric currents, by galvanic currents, and by ammoniacal pharyngien shocks.*"—It is referred to the committee of medicine and surgery.

M. Linari explains that his want of familiarity with the French language had caused his reclamation* to be misunderstood. He states that M. Melloni has always given him his due share of credit; but that he objects to M. Palmieri having attributed to himself their joint discoveries of the spark, and other phenomena of telluric induction, and the *magneto-electro-telluric battery*, by which these phenomena are produced.

July 21.—M. Ch. Matteucci "*On Electro-Static Induction or the Discharge of the Jar.*" (*Vide* No. IX., *Elec. Mag.* p. 21.)

M. Sellier addresses a new† communication relative to the *Preserving effects of black bodies considered as Conductors of Electricity.* The contents of the communication are not given.

July 30.—M. Arago announced that Prof. Faraday, one of the eight Foreign Associates of the Academy, was present assisting at this meeting; and he presents a copy of Mr. Faraday's late memoir "*On the Liquefaction and Solidification of Bodies generally existing as Gases.*"

We have made an extract from our own copy of this paper, which is given in the last Number at p. 158.

Report on MM. Gaultier de Claubry *and* Dechaud's *Memoir*‡ *concerning the Electro-Chemical Treatment of Copper-Ores.* (Committee, MM. Berthier, Dumas, and Becquérel, reporter.)—After describing the process adopted, the report says that it "appears to present advantages over the ancient modes of treatment; but it requires that the ores be transformed entirely and chiefly into sulphate; the whole industrial question is in this. On the other hand, the partially exhausted solution of copper becomes more and more charged with sulphate of iron; so that, in passing it anew over the ores, to saturate it with the sulphate, and returning it to the apparatus, a time arrives when the quantity of iron it contains is such that the precipitated copper contains a certain proportion of this metal, which alters its quality. To prevent this inconvenience, it is necessary not to re-pass the two ferriferous solutions upon the ore, and to precipitate the copper which they contain by iron."

The committee conclude by expressing a belief "that this application on a large scale of the electro-chemical process for treat-

* *Vide* last No. p. 145.

† Vide *Elec. Mag.* Vol. I. p. 311.

‡ *Vide supra*, p. 103.

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ing copper-ores, presents chances of success;" and they invite the authors of the process "to make known the results, that they may obtain in the different establishments which they are fitting up."

Observations on the Decomposition of Water by Metals, under the Influence of very small proportions of different Metallic Solutions. By M. C. Barreswil.

The author refers to a paper by M. Millon, read on July 7, and thinks that zinc, tin, or lead, for instance, are more attackable by hydrochloric acid and a few drops of a salt of platinum, because the latter metal becomes reduced, and thus forms a veritable voltaic element; that arsenious acid accelerates the decomposition of water by zinc, and retards it by iron, because in the one case the reduced deposit is porous, and, in the other, it is continuous and impermeable, like a varnish. And so in other cases.

August 11.—*Death produced by an Electric Discharge, which does not appear to have been accompanied with any Detonation.* Letter from M. Regnier to M. Merimee.

M. Regnier described this circumstance about thirty years ago; it occurred $1\frac{1}{4}$ mile from Chailly: it has lately engaged the attention of M. Arago; and M. Regnier greatly regrets that he cannot lay his hands upon the original notes; so that he is compelled, much against his will, to have recourse to his memory. A girl eighteen or twenty years of age was reaping with her mother and father, in hot, dry weather. At three P.M., the father saw a cloud approaching, and expecting a storm to be at hand he sent his daughter homeward, he and his wife intending to overtake her. The girl set out in a direction toward the cloud; and the father, in a few moments, turned to see what had become of her, and was surprised to see her a few paces from him, lying on her face; on approaching, he found her motionless and lifeless. Three hours after the event M. Regnier accompanied the *juge de paix* to the field, where the girl still lay in the position, as they told him, in which she had been found. Her arms were not forward, as occurs in ordinary falls, when time is given for this instinctive action; her dress was not raised, but her bonnet was three or four paces from her. On examining the body I saw no hurts except that the hair of the right groin was reddened, as if by a taper, but the skin retained its natural colours; the right arm-pit presented the same phenomena; and a few drops of blood were in the right ear. The death, therefore, could not be attributed to wounds, neither could it be simple apoplexy; for in this case the blood is in the cranium and not at the extremities; besides, the singeing of the hair excludes this idea. He says, "I arrived by the way of elimination to attribute the death to lightning, notwithstanding the declaration of the father of the young girl, who affirmed to me that he did not hear the slightest noise." The author was led to think that the electricity rose from the earth, because the bonnet was driven off and not on, and the marks on it resembled those which a stick with a rounded end might give when acting from below upward. He does not remember whether the limbs were supple or rigid; the body was not opened.

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M. Dujardin writes concerning a process, conceived by M. MAUS, a Belgian engineer, of employing *the Electrical Telegraph for Controlling the Progress of Railway-Trains.* By a simple arrangement (which is not given in the report), the place of the train would be at all times known, and also the time it had taken in going from place to place.

August 18.—M. JUVIOLI "*On Electro-Vegetation.*" (*Vide Elec. Mag.* No. 10, p. 160.)

August 25.—*On the Trombe of Melaunay and Morville.*—Letter to M. ARAGO by MM. NELL DE BREAUTE, and PREISSER. See the *Comptes Rendus.*

September 7.—*Employment of the light produced by the pile in a detonating Atmosphere.* Letter from M. BOUSSINGAULT to M. ARAGO.

At a period when men are actively thinking of applying the voltaic pile for the illumination of towns, it is permitted to hope that soon subterranean works will receive a light that grows and maintains itself in vacuo, without its being necessary for its support to nourish a focus of combustion with an atmosphere that is but too frequently explosive. Grove's pile, and that of Bunsen, which, with 48 pairs only, and consuming per hour but 1^{f} 20^{c} (one shilling) of materials, gives a light equal to that of 500 or 600 stearine candles, seem to resolve the question of economy, however great may be the portion of doubt inherent to valuations of this kind. Let us not forget, moreover, that beside the industrial question, is found a question of humanity: in Europe, one man per day perishes by fire damp.

Not having at my disposal a Bunsen's pile, I used, in my experiments, a pile of zinc and copper elements, constructed with singular perfection by M. Münch, director of the Industrial School of Strasburg. With this pile I convinced myself that the current established between two charcoal points, placed either in vacuo or in water, produces a jet of light, which may be carried, without the slightest fear, into a denotating atmosphere.

Would it be worth while to construct smaller portable apparatus giving just sufficient light to illuminate a cutting? or would it even be more advantageous to establish in the works, at the well ventilated points, intense flames, where the light might be distributed by means of reflectors?

I touch here a subject of photometry which especially concerns yourself. If I have not given greater extent to my experiments, it is because, mine not being a constant pile, I do not obtain a sufficiently constant light. I propose returning to my experiments, when I shall have been able to procure suitable apparatus, and especially when I can have the advantage of the kind councils of my worthy fellow-academicians.

M. GARNIER submits to the Academy an *Apparatus for the Electric Telegraph*, in which the author is principally occupied in contriving to produce the signs very rapidly.

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M. LABORDE addresses a note relating to *Signals for the Electric Telegraph.* He gives the signal by simple or triple blows struck on a bell.

An Improvement on the Magneto-Electric Machine. By M. DAJARDIN, of Lille.

This improvement consists in turning between the branches of a horse-shoe magnet, and in the interior of a large fixed bobbin, two prisms, one of soft iron, the other of lead, screwed on a brass axis parallel to this axis. The lead prism merely serves as a counterpoise to that of iron. During the rotation of the system, the ends of the iron prism pass very near to the poles of the magnet, and they are then removed to a distance equal to the thickness of the axis. These alternate approaches and removes determine induced currents of remarkable tension in a copper wire 2,000 metres in length, which is rolled on the fixed bobbin. A magneto-electric machine of this kind was compared with a good Clarke's machine by means of two tubes graduated in centimetres, and filled with filtered rain water, at the zeros of which the polar wires of the two machines were successively brought. The shocks of Clarke's machine, the magnet of which had been re-magnetized, ceased to be perceptible at 34 c.m., whilst those of my machine did not cease to be felt until at 87 c.m.

Sept. 8.—*On the Meteor of Melauney.* By M. POUILLET, pp. 15.—Our readers are referred to the *Comptes Rendus* for this Memoir.

New Experiments on the Torpedo. By M. MATTEUCCI. (*Vide* the present Number, p. 198.)

Sept. 15.—*On the Illumination of Mines by means of the Electric Lamp.* Letter from M. DE LA RIVE to M. Boussingault.

I see by the *Comptes Rendus des Séances de l'Académie*, that you* are occupied in seeking means to employ the voltaic pile for lighting mines. This subject has also occupied me for a long time. I have made several attempts, which have not all been equally successful, but latterly I have met with more success, and I am on the way to a process, which I believe to be at once economical, and very convenient. The pile that I employ is formed of several concentric cylinders of *copper* or *platinum*, separated from each other by porous cylinders, so as to form four or five pairs in series, the positive metal is a liquid amalgam of zinc, and, still better, an amalgam of potassium; the liquid is a solution of sulphate of copper in the case where the negative metal is copper, and chloride of platinum in the case where it is platinum.

One of the greatest difficulties is to obtain constancy in the light. I have not yet perfectly attained to this; however, I have already gained much by employing small, thin, hollow cylinders of coke, analogous to those employed in Bunsen's piles, except that their dimensions are much less, and arranging these cylinders like the wicks in a lamp. A thick ring or disc of metal, of the same

* *Vide supra*, p. 217.

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diameter as the charcoal cylinder is arranged below this latter, so that the electric current escapes between the two. Care must be taken that the current passes from the charcoal cylinder, which is below, to the metallic conductor, which is above, in order that the particles of charcoal transported from below upward, may fall again by their own weight. The whole, namely, the charcoal cylinder and the metal appendages which support it, as well as the ring or disc serving as a conductor, are placed in a small glass globe hermetically closed. It is not necessary to make a vacuum, because the little oxygen contained in it is very quickly absorbed by the incandescent charcoal; but care must be taken that all communication with the exterior air be well intercepted. With regard to the pile, it is adjusted outside the globe by two metal rods, which communicate, one with the charcoal cylinder, the other with the metal conductor. It may be changed or charged anew, without in any way deranging the interior.

According to the power of the piles, it is proper to employ two charcoal points or cylinders, rather than a simple metal conductor. The preparation of the charcoal is also a point of great importance; I have made several attempts on this point, and I am not yet completely satisfied.*

Sept. 29.—*Electric Telegraphy.* By M. Breguet.

This was an account of experiments made on the Paris and Rouen line of telegraph, the results of which confirm the position that has been advanced by several philosophers, that "the resistance of the earth as a conductor must be very small, or almost nothing, when compared with that of a metal conductor of the same length." The author concludes from some of his experiments that mere contact with the smallest metal surface and the earth is sufficient for giving the maximum effect.

M. Moltoni addresses an explanation of *A New System of the Fabrication of Compasses.* His communication is referred to a committee.

Mr. Eben Meriam, of Brooklyn, writes to M. Arago that, in the last three years, about 150 persons have been killed by lightning in the United States.

By a letter from Mr. David Henshaw, Minister of Marine in the United States, to Mr. Meriam, it appears *that there has not been a single example of a ship of the Union provided with a lightning conductor having been struck with lightning.* Mr. Henshaw's

* In the *Phil. Mag.* for December, is a letter from Prof. Grove, in which this paper is referred to. Mr. Grove thinks there are too many difficulties to overcome with the charcoal light. He has made many attempts with it, but his success has been but partial. He has met with more success by employing platinum spirals, which he protected in a bell-glass over water. He says:—"the spirals of the helix should be as nearly approximated as possible, as each aids by its own heat that of its neighbour, or rather diminishes the cooling effect of the gaseous atmosphere." By an arrangement of this kind he was able to experiment and to read for several hours: he used three or four pairs of the nitric acid battery. Mr. Grove also gives the results of some experiments in several gases.—*Editor.*

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letter is dated August 2, 1843. Mr. Eben Meriam thinks it would be useful to preserve the metallic brilliancy of the conductors of lightning-rods.

October 13. — *Memoir on Clocks, Telegraphs, and Electric Logs.* By ALEXANDER BAIN.—The log is a modification of Massey's. The electric apparatus consists of a wire connected with the compass; and the sea is the exciting liquid. At every quarter of a mile a pen or pencil marks the course of the ship.

New Electro-Magnetic Apparatus. By M. DUJARDIN.—This, or a machine very similar, appears to have been described at a previous meeting of the Academy. (*Vide supra*, p. 218.)

On M. Theyer's Process of Copying Designs by Galvanography. By M. PELIGOT.—The drawing is made on paper by a pen or pencil, the ink or pencil being of a particular composition. This is transferred to a copperplate, and is thence copied by electrotype. About 500 impressions may be struck off. The difference between this and M. Kobell's process is, that in the one case the drawing is made on paper, in the other on copper.

M. ARAGO submits to the Academy *Mr. Bain's instruments*, described at the last meeting.

M. DESBŒUF POTTIER writes relative to M. Regnier's note, respecting the young girl who was killed by lightning, without any noise of thunder having being heard. The author wishes to see the said note, as the report which has reached him by the journals seemed inaccurate. He is referred to the *Comptes Rendus*.

October 17.— *Experiments relating to the Heating of a Metal Conductor, uniting the two Poles of a Pile.* By M. VON BREDA. (*Vide* present Number, p. 205).

November 3.— *On a New Method of Curing certain Aneurisms, without Operation, by means of Galvano Puncture.* By M. Jo. PETREQUIN. (*Vide* present Number, p. 206).

M. ARAGO laid before the Academy the apparatus by means of which *the Signals to Morse's Telegraph* are made.

REVIEWS AND NOTICES.

Elective Polarity, the Universal Agent. By FRANCES BARBARA BURTON.—Simpkin and Co. 1845. 8vo. pp. 171.

WE know that a certain "scholastikos" is reported to have carried about with him a brick, as a sample of a house he had for sale; by some such process we must treat the present volume. *Imprimis:* we have not read the book,—we were fairly vanquished after the first chapter or two: we confess, to our shame perhaps, that we are not equal to the task; the fair authoress soars into regions beyond our power to follow, leaving us in mute amazement at the reckless rapidity of her flight;—we ask ourselves, Where is she? and echo answers, Where?

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We derived some consolation, however, from p. 32, by seeing that we are not alone in this our impotence of mind.

"Into sublimities like these, which render astronomy the sublimest of studies, and the most redolent of devotion, Sir Isaac Newton could not enter. The utmost grandeur with which he could invest the universe was that of a splendid toy-shop. . . . . One might as well look at Jupiter through a microscope, as study astronomy under Sir Isaac Newton's auspices."

The book opens; by telling us, that 12,500 years ago Vega, in Lyra, "poured its magnetic streams throughout the earth's planet;" and this is argued to be the period when the gigantic fossil animals occupied our earth: and it appears that man's race is to be extinct, or greatly altered, when Vega shall next perform the same kindly office,—as saith the following:—

"And, since the stamina engrafted by the influence of Ursa Minor could now withstand the stupendous energies of Vega, it follows that Man—or what race soever may then represent his supremacy on this planet—must be gifted with far wider faculties," &c. &c.

The curious inquirer must search for himself for the nature of these "stamina," and their *modus operandi*. And perhaps he may feel the more induced to undertake this investigation, if we give him the definition of this *modus operandi*,—this *Elective Polarity*:—

"By Elective Polarity is, therefore, understood effluvial impetuses of ineffable activity, which circulate throughout all organizations; each emanation of which comprising certain elementary or magnetic properties, in accordance with those angular and polaric diversities inherent in its classification, and its activity being determined, as before stated, by the proportional polaric obliquity and angular acuteness of its circulating qualities." P. 21.

We strongly suspect that, if the spirit of Sir Isaac were to arise, he would candidly confess that he could not, indeed, enter "into sublimities like these." Now for a few electrical passages, which may perhaps electrify our readers, and we have done. We will pass over "the purity of magnetic emanations," and "the grand hues of electric effulgencies,"—the "magnetic effluvia," and the "electric emanations,"—all which are as familiar as household words to our authoress. We pass over such common-place matters as these, and content ourselves with giving our readers one more peep into these regions of transcendentalism.

"When our investigator traverses his zodiacal section in the morning, he finds his position *meet the effluxial action;* that is, are under the invigorations of *positive* magnetic polarity in direct transfusion upon them. In the evening, when he and his position again traverse a section of it, it is in an opposite direction. They are *then* leaving the zodiacal effluxes behind them,—the earth turning *downwards from* them. Consequently, *their* polarity, and those of effluxes then in action, are relatively *reversed*. Our investigator is then under the influence of NEGATIVE magnetic polarity. In the morning, his automatic functions instinctively *expand* for reception of *positive* magnetic vitality, and our investigator unbuttons his coat. In the evening, his automatic functions instinctively *contract*, in counteraction of undue magnetic exhalations from his system through the extraction of *negative* magnetic polarity, and our investigator buttons *up* his coat." p. 164.

From this very lucid passage, we come to the important con-

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clusion, that our England, in the year of grace 1845, must have taken, or have been taken, the wrong road throughout the spring season; and that it never met *the effluxial action* until May 29th, for, until that day, we were very glad *to button up our coats* every *morning* as well as evening. In conclusion, we should advise every one, who wishes to *seem* wise above his neighbours, to place this book on the drawing-room table. Once *persuade* them that he has read these pages, and *understands* them, and his fame is established. The book is a very respectable-looking book, in blue cloth boards, printed with clear type, on good paper. When the translation into the "vulgate" appears, we will not fail to remind our readers of it.

Trattato del Magnetismo e della Elettricità. Dell' Ab. Fr. ZANTEDESCHI, Prof. di Fisica, &c. nell' J. R. Liceo de Vicenza, &c. Part I. Nov. 9, 1844, pp. 389; Part II. 1845, pp. 546. 8vo. Plates.

THIS treatise evinces on the part of the author a great amount of knowledge, of patience, and of industry. It will be a useful manual to us in England, inasmuch as the author appears to have had access to a vast number of Italian memoirs, which are comparatively little known here. It is undoubtedly an *Italian* treatise; a treatise, that is, in which the labours of his compatriots are not the least prominent features in the progress of the science; but we do not complain of this; it could not well be otherwise; for we all must talk and write on those matters which are most before us, and most familiar to us. The references throughout are very copious; and they disclose to us many treasures that might have cost us some pains had we been compelled to search after them ourselves. We were going to press when the second volume came to hand; so that we have not had time to go carefully through it. We see that the last forty pages are devôted to the therapeutic applications of electricity. We shall hereafter give extracts from these volumes; for the present, it will suffice hastily to run over the plan that is adopted.

It is divided into ten sections, of which the first part contains four, and the second part six. They are in order; 1. Magnetism; 2. Common Electricity; 3. Atmospheric Electricity; 4. Galvanic and Voltaic Electricity; 5. Electro-Magnetism; 6. Thermo- and Luci- Electricity; 7. Electric-electro-dynamics; 8. Animal Electricity; 9. Magneto-Electricity; 10. Electrotype and Therapeutic Electricity. It may perhaps be advisable to mention that, in the 7th section, under the somewhat untranslatable term, *Elettro-elettrico-dinamico*, he includes induced electric currents, from whatever source they originate. The fourth section contains much information concerning the early history of voltaic electricity; with several original passages extracted from memoirs then published. A great many of the most recent discoveries are interspersed throughout the book, which afford ample evidence that, if the author is not intimately acquainted with all that has been done

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elsewhere, he at least knows very much; but we must forbear for the present entering further into the volumes.

NOTICES OF EXPERIMENTS, &c.

Static Induction.—"It now remains to us to prove by experiment that there really exist these alternately positive and negative electrical states in the interior of an insulating mass, between two conductors charged with opposite electricity. The most distinct experiment is this, which I will make for you, with a pile of very thin laminæ of talc, placed one above the other, and coated like the magic picture with two tin discs on the opposite faces. Observe that I place one of the extremities of this pile in communication with the conductor of the electrical machine, and the other in communication with the ground, in order to condense into these opposite parts the two electricities of contrary names. I then carry these laminæ separately one by one to Bonenberger's electroscope, and I perceive that each lamina has the two electricities separated upon the two opposite surfaces, the positive upon the surface that was turned toward the conductor, and the negative upon that which looked toward the ground.

This experiment may also be made by fixing two handles of shell-lac to the two opposite surfaces of a crystal of mica, and then separating the crystal into laminæ, by drawing the two handles in opposite directions, after having employed the electrical machine. But in this experiment it would be extremely difficult to avoid errors caused by the friction of the laminæ."—Signr. R. Felici's Report of a Lecture by Prof. Matteucci, at the University of Pisa.—"*Il Cimento*," 2nd No. for 1845, p. 147.

Separation of Metal from Ores.—Mr. Napier's process, noticed in our report of his lecture,* is thus specified:—"I take a large crucible or other convenient vessel made of an electro-conducting material, those I have used being of plumbago (common black lead); the inside of this vessel I line all round with a lute of clay, except the bottom which I leave uncovered; the luting should be very thin, and laid on in two or three coats, drying slowly between each so as to prevent cracking. When the vessel is sufficiently luted and dried, I put therein (with the usual fluxes) the regulus or calcined ore, which, when sulphurets are used, should have been well roasted so as to drive off as much as possible of the sulphur. I then place the vessel with its contents in an ordinary air-furnace, keeping up the heat until the mass is in a state of fusion. In the mean time I have prepared an ordinary voltaic battery of copper and amalgamated zinc charged with acidulated water, one part sulphuric acid, twenty-five parts water. To the positive wire of this battery I attach an iron rod, having riveted at right angles to its extremity a flat disc of iron, the disc being a little smaller than

* Vide *Elec. Mag.* Vol. II. p. 44.

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in [is?] the inner circumference of the crucible. To the negative wire of the battery I attach a simple rod or bar of iron. The matter in the crucible being in a state of fusion and well fluxed, I place the above-mentioned disc of iron, which will now form the positive pole of the battery, on the surface of the fused mass, and keep the rod, which is connected with the negative pole, in contact with the outside of the crucible, the bottom of which thus forms the negative pole. The fused matter now forms a portion of the electric circuit, and the heat being kept up the metal is gradually reduced and deposited at the bottom of the crucible. The proportions which I have found suitable are as follows:—For every hundredweight of regulus of thirty per cent. of metal I employ a battery of five pairs of plates, the size of [the] zinc plates being three feet square, and the copper doubled round the zinc in the usual way. The size of the poles should be smaller than that of the zinc plates. I have found about half the size answer well. With apparatus of these proportions, the time for extracting the metal varies from one to two hours. The power of the voltaic battery, or other means of generating electricity which may be used, will vary for different ores, and I have found that, if a larger battery be employed with the same quantity of ore, the time required for the extraction of the metal will be shorter, and *vice versâ*; and if a more powerful voltaic combination be used a smaller-sized battery or a shorter time will suffice. Experience will enable the operator to regulate these matters so as to produce a good result. I have found that by merely connecting, by an external good electro-conductor, the iron on the surface of the fused mass, and the bottom of the crucible, an electric circuit is formed, and metal is deposited, and the result is not so satisfactory as when the battery is employed; whether a crucible or other vessel or furnace be employed to contain the fused ore, it should have its bottom, or a sufficient portion of it at the lowest part, formed in the interior of plumbago or other suitable electro-conducting material with which the negative rod can come in contact. The positive pole also should consist of an electro-conducting material; and I have found iron as above described the most convenient and satisfactory, although I have also found that copper may be used. The thickness of the wires or straps of metal used for connection should increase with the size of the apparatus to the proportion above given; I have found straps of ordinary sheet copper of an inch broad sufficient. The metal so extracted may be refined, when necessary, in the usual way. — *Sealed* Oct. 22, 1844; *enrolled* April 22, 1845. *Repertory of Patent Inventions*, July, 1845.

Phosphorescence.—"Some singular appearances I have noticed on box-tree leaves. Upon dark, cloudy, and warm nights, after rain, and while yet the drops remain upon the leaves, each individual leaf lights up with a pale, phosphorescent light, not very dissimilar to that of the glow-worm, save that it throws out no light beyond the leaf itself. It does *not* present itself on the

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pointed top of the leaf, but is seen through the water-drop as through a lens; by many precautions I satisfied myself that it arose from no reflected light. A man, who lived in my employ previous to my quitting Gloucestershire, informed me that he had *once* seen a similar appearance upon some trees in a beech grove, about one o'clock on a summer's morning, after a thunder-storm." —*Extract from a note to* C. V. W. *from* Rev. M. A. Hartnell.

Magnetization. — A horse-shoe magnet, of eight plates, and weighing eight pounds, sustained thirty pounds; it was made as hard as possible; and when now remagnetized supported but twenty-eight pounds. By Dr. Scoresby's advice, the plates were separately boiled in linseed oil; after which it would sustain forty-four pounds. The boiling has somewhat disarranged the plane surface of the poles, or it would doubtless sustain more.—*Note to Editor from Mr. T. Dury.*

Time and the Electric Telegraph.—We have heard of things being done "in less than no time," and always looked on the phrase as a figure of speech signifying great despatch. This paradox seems, however, to have been actually realized in the case of Wheatstone's Great Western Telegraph, a message having been sent in the year 1845, and received in the year 1844! It appears that, directly after the clock had struck twelve, on the night of the 31st of December last, the superintendent at Paddington, signalled his brother at Slough, that he wished him a happy new year; an answer was immediately returned, suggesting that the wish was premature, as the new year had not yet arrived at Slough! Such, indeed, was the fact; for "panting time," was matched against Prof. Wheatstone, and beaten half a minute.—*Reading Mercury.*

Electro-Motive power of Light.—When two iodized silver plates are immersed in water, in the dark, being at the same time connected with a galvanometer, no deflection occurs; but the needle moves immediately on a ray of light being allowed to fall upon the iodized side of one of the plates. This plate becomes positive.—*Becquérel. Poggendorff's Annalen*, Vol. lv. p. 588.

Telegraphy across the Channel.—In reply to a note of ours on the subject, Prof. Matteucci has enabled us to remove the obscurity from his suggestion, as given in pp. 41 and 42 of the present volume of *The Electrical Magazine.* Of the two metal arcs alluded to, one is supposed to be in France, for instance, and the other in England; the metal, at one end of the French arc, is immersed in the sea at Calais; the metal at the other end is in a very deep bricked pit, at a considerable distance from the coast; the battery may be in this arc. In like manner, one metal of the English arc is in the sea at Dover, and the other in an inland pit; the galvanometer or other telegraphic apparatus may be in this arc. There are, therefore, two paths for completing the circuit; the first is from the plate immersed in the sea at Calais to the plate buried in the pit in France; the second is from the plate immersed

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at Calais to that immersed at Dover, along the arc to the plate buried in England, and from that plate, under the Channel, to the plate buried in France; it is theoretically possible so to adjust things that a good portion of the current should pass by the latter path; but the obstacles to the practical application of this theory appear almost insuperable.—*Ed.*

Lightning.—On Friday, May 10, 1845, a storm, in which but one flash of lightning was seen, and but one peal of thunder heard, occurred at Walton, near Stafford. The flash struck the spire of the church, and destroyed the greater part; it melted the iron spouts on the south-west side, and the glass and lead of the windows; it shattered the window-sills and a portion of the roof. Some men, working near the church, were not injured. On the following day, at Bristol, the lightning entered a drawing-room and struck a lady on the head, causing a lump to rise instantaneously; the sight of the left eye was gone for a considerable time, and the mouth was drawn aside as if by paralysis. She soon, however, recovered, retaining only a numbness on the left side. At nearly the same moment, the lightning entered the room of a house five miles from Bristol, in which was the sister of the lady above mentioned; the paper was torn from the walls, and the skirting boards were torn away; and a gun that was in the room, and loaded with powder only, was discharged.

Death by Lightning.—On Thursday, April 24th, two labourers, who had been hoeing in a wheat-field at Chigwell, took shelter beneath some oaks from a storm. At one o'clock, after having previously heard four claps of thunder, they were both struck. One of them was rendered insensible; and, on his recovery, he found himself on the ground, and his unfortunate companion lifeless by his side. His eyebrows were singed off, as was also his right whisker, and a shilling in his right-hand trousers pocket, was perfectly indented.—*Chelmsford Chronicle*, May 2d, 1845.

Intellectual Electricity.—"Are not persons reciprocally in love and beloved, a kind of electrified machine, ignited both negatively and positively, not only to each touch, but also at each glance and motion? . . . . And the negative colour, *black*, probably imbibing electric fire most powerfully, witness the spark issuing from black animals; are not dark eyes the readiest recipients and conductors of passion, being surcharged, lambent, and overflowing with genuine flame, whilst others often but glitter and twinkle? . . .

> "Men are warned 'gainst black eyes,
> That absorb, as it flies,
> Each oxygenous spark,
> By day and by dark;
> That, with venom replete,
> Murder all whom they meet;
> And, lambent with flame
> Hearts, the toughest, can tame."

—Grave extracts from a grave book, with the above title, published in 1798.

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Electric Gun.—This instrument is by Mr. Beningfield, and has been exhibited for the last four months in Jersey, not *New* Jersey, as implied in the notice we gave in our last Number (p. 614). We were present on Saturday, June 14th, in King-street, Westminster, to witness its effects; but whether it is called an "Electric Gun" by a figure of speech, or from its really depending on some electric arrangements, we are unable to say. The inventor keeps his own secret; and all that we see of its action is, that, when a lever is touched, the balls are violently propelled, accompanied with a slight puff of steam. After its effects were shown, it remained quiescent, and we were told that it might be left so for weeks, and be ready for action at any required moment: when out of action, *the connections were said to be broken.* In reply to all questions, we are referred to the handbill.

Electric Telegraph.—M. Lomond "is a very ingenious and inventive mechanic. In electricity he has made a remarkable discovery. You write two or three words on a paper; he takes it with him into a room, and turns a machine enclosed in a cylindrical case, at the top of which is an electrometer, a small fine pith ball; a wire connects with a similar cylinder and electrometer in a distant apartment; and his wife, by remarking the corresponding motions of the ball, writes down the words they indicate; from which it appears he has formed an alphabet of motions. As the length of the wire makes no difference in the effect, a correspondence might be carried on at any distance. Whatever the use may be, the invention is beautiful."—*Young's Travels in France*, vol. i. p. 65. 1792. Vide *Athenæum*, Aug. 23d, 1845.

Electrolysis affected by Solar Light.—"A small galvanic arrangement having been made by placing a tube, closed at one end with a skin diaphragm, in a large test glass, a solution of the iodide of potassium was poured into the tube, and a solution of nitrate of silver was put in the outer glass,—the two fluids being connected with a piece of platinum wire. An arrangement of this kind being placed in the dark, in about four hours a very beautiful crystallization of metallic silver was found to have taken place about the platinum wire, in the silver solution. Another arrangement, in every respect prepared in the same manner, was exposed to sunshine. Not only did no metallic crystallization take place whilst exposed, but the exposure had so changed the character of the solution, that no such crystallization would afterwards take place, even in darkness. This experiment was many times repeated, with various modifications, but always with the same result. Here we have a very striking instance of the interference of the solar radiations, with the exercise of the force of galvanic electricity. It is not improbable that these may be made measurers of each other. Knowing the force of the current generated between the two solutions, it will not be difficult to ascertain the quantity of the solar rays necessary to retard or stop the action of that force."—

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Mr. Robert Hunt. *Cornwall Polytechnic Report* for 1844, p. 103.

Galvanic Rings.—"All persons afflicted with imaginary diseases are earnestly recommended by *Punch* to try a galvanic ring. It signifies not whose ring, out of the large variety advertized, they may select, since the virtues of all are equal. These rings consist of a thin wire of zinc, soldered to a similar one of copper; hence the quantity of galvanism they generate is next to nothing; certainly much less than that produced by a sixpence and a half sovereign lying in contact in a gentleman's waistcoat pocket. Its exact amount it would be difficult to compute, as the galvanic fluid has no weight: but, giving a rough guess, we should say that it may be about the decillionth part of a suspicion. The tremendous effects on the system of this strong homœopathic amount of galvanism passing through the little finger may be conceived. But, to quiet alarm, we may state that none whatever of this frightful torrent of galvanism, developed by the ring, passes through the finger at all. To be affected by galvanism, it is necessary that the part to be galvanized should lie between two opposite galvanic poles, or extremities: now, as the finger is inside the ring, the galvanism is outside the finger. Thus the finger is to the galvanic fluid as a person standing on the Isle of Dogs is to the fluid in the Thames. With what power an inappreciable force, ineffectually applied to the extremity of the body must act in the cure of such a complaint as rheumatism, is obvious. In all positive diseases, the galvanic ring will afford a negative relief; and those who have nothing the matter with them may confidently depend on being cured by it."—*Punch*, April 12th, 1845.

The galvanic ring folly had come to such a pass in these our boasted days of mental illumination, that our witty friend *Punch* actually adopts, in the above extract, more of the grave than of the jocose strain. We hear that, in addition to the whimsies already noted, many Anglican hose are now sustained by galvanic cinctures. Farewell gout! Vive, we presume, la Polka!—Ed.

Electro-Culture.—"The electrified shoot of the apple-tree, decribed at p. 112 of last Number of *Electrical Magazine*, continues healthy and vigorous, and has thrown out eight lateral shoots, stouter and larger than any on the other parts of the tree, the eleven remaining shoots of which present but seven laterals altogether, some of which are dead from canker, and others looking very sickly.—*Note to Editor from W. H. Weeks, Esq.* Dated *Sandwich*, Dec. 17th.

Heating Power.—At the late meeting of the Italian Society at Milan, Sept. 24th, 1844, Prof. Botto detailed the experiments that were fitted for establishing the law, announced by Joule, and confirmed by Ed. Becquérel, that the heat developed by the electric current in a conducting wire is in direct compound ratio of the resistance of the wire, and of the square of the electrolytic action. —*Il Cimento*, Aug. 3d, 1845, p. 66.

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Blasting by Galvanism.—The Corporation of London have been engaged in removing part of the Whiting shoal, in Limehouse-reach, by galvanic agency, under the direction of Capt. Fisher, principal harbour-master. The first explosion was made on Tuesday, May 20th, of two canisters of powder, each containing 46 lbs. On passing the voltaic current, the water rose in a dome-like form, and it was found that forty feet of the shoal was shattered, and fifteen tons or more of rubbish were released.

Charm against Lightning.—"Behind the door you will generally find a branch of box, which was blessed on Palm Sunday, and which has the property of protecting the house from lightning and from fire. Sometimes every room is thus protected; and, in case of a violent storm, the master or mistress of the house has recourse to the "bois beni," dips it in holy water, sprinkling the room, and all the parties present, after which they all kneel in prayer." —*Superstitions of Tauraine. Edinburgh Journal*, March 29th, 1845.

Galvanic Rings.—"The sellers of them assert that they can do no harm, if they do no good. This is not altogether true. A medical friend of ours was called in the other day to a poor man who had worn one of these rings for rheumatism, and found his finger swollen and inflamed; so that in this case much unnecessary pain, and the loss of a week's wages, we presume, was the result of the experiment."—*Chambers' Journal*, April 26th, 1845.

Attempt at Suicide, and wonderful effects of Electricity.—A Mr. Richard Fowler, an inhabitant of Mansfield, arrived in Nottingham, on his way to Belton fair, having left Mansfield that day, He called at the Reindeer Inn, in Wheelergate, and proceeded to the Talbot Inn, and expressed his intention of stopping all night. He had nothing whatever in the shape of drink, and retired to bed about ten o'clock. In the morning the "boots" went to call him, and could not make him hear, and on trying the door, found that something was placed against it. He accordingly pushed it on one side, and found it was a chair, which had been purposely placed there, and on looking at Mr. Fowler, who lay in bed, he felt convinced that he was dead, which fears were increased on shaking him, and finding he did not move. The "boots" immeditaely told Mr. Weaver, who directly sent for Dr. Hutchinson, and that gentleman promptly attended. He was soon convinced that the inanimate man was suffering from the effects of poison, and the aid of Mr. Sibson, House-surgeon to the General Hospital, was called in, along with the dispenser to the Hospital, Mr. Sheperley. Mr. Sibson soon arrived, and active measures were taken for expelling from the stomach the poisonous liquid. An emetic was administered, but it failed in producing any effect—the stomach pump did not bring up the least particle of poison, and it became evident, after repeated attempts, that the whole had become absorbed in his system. During the whole of this time, Mr. Fowler lay motionless,

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and to all appearance dead, and as a last resource, it was determined to try the effects of electricity. A shock was administered by means of the General Hospital electrifying machine, and at the first application of the conducting wire to the chest of the patient, he immediately rose up in bed, but gradually fell back again. At the second shock he again rose up in bed, loudly crying out "Oh," and then again fell back; but at the third shock he started back more quickly than before, crying out, "Oh God!" and sat upright with ease. In a short time afterwards he asked for something to drink, and tea and coffee were administered to him; in half an hour he was enabled to get up and walk, and in three quarters of an hour he dressed himself, and appeared almost entirely recovered. He then gave an account of what he had done. That he had purchased two ounces of laudanum at Mansfield, with the intention of taking it at Nottingham, and that on retiring to bed he had taken half of it, but finding it did not have the full effect, he had taken the remainder. The bottle was found on the floor, labelled "Laudanum, poison," with a Mansfield druggist's label. On the following day, he was conveyed to the lunatic asylum, as it was evident his mind was still unsettled, where he now remains.—*Local Paper.*

St. Elmo's fire.—"On May 16, 1844, a traveller, who was passing by night along the sands between Pontorson and Mount St. Michel, perceived that he was surrounded by electric lights. This fact, which has often been observed, induced M. de la Pylaie to revert to one very nearly similar; but which was produced under unusual circumstances. In the isle of Moirmontier, lights of the same nature surrounded the head of a woman, who was at the time within side her house."—*Comptes Rendus*, June 3d, 1844.

Electricity and Vegetation.—I have tried, during the present year, the effects of the arrangement described as electro-culture (the parallelogram of wire) and galvanic culture, a large plate of zinc at one end of a bed of potatoes and a pit full of coke at the other, connected by a suspended wire. I confess I expected nothing from the first arrangement, but I do not think *either* has produced any result, at least I am not satisfied that the condition of the crop is really consequent on the electric arrangement. We have to-day been digging a potato bed thus treated, but from which all the blossoms had also been pinched off, and I must confess that though the produce is rather small in size, the tubers do not appear at all (or scarcely) affected by the prevalent disease, whilst in another ground not thus treated nine-tenths of the produce (fine looking and large potatoes) are utterly valueless."—*Note from* Rev. F. Lockey *to* Editor, *dated* Oct. 20, 1845.

Influence of Electricity over Evaporation.—Mr. Rowles suspended two similar capsules of water by silk strings over a stove that was daily heated; one of which he connected with the earth by a copper wire. In 24 hours, 2 oz. 279 grs. had evaporated from the

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former, and 3 oz. 144 grs. from the latter. The like occurred in the heat of the sun.—*Phil. Mag.* Vol. xx. p. 45.

Electricity of Steam.—Prof. Zantedeschi communicated to the French Academy, March 31, 1845, that "having formed a jet of steam, by an opening in a tubular boiler, he applied the electrometer to it; and ascertained that, at the origin of the jet, the electricity was negative, at a certain distance it was positive, and between the two points was a zone, in which the steam gave no indication of electricity."—*Athenæum*, April 12. Our readers have long ago been made familiar with the whole philosophy of these effects from Faraday's able researches reported in Nos. 1 & 3.—*Ed. Elec. Mag.*

Electrical Clock.—"Mr. Samuel Thompson, of Darlington, has completed an electrical clock to go for years without requiring to be wound up, it having neither weights nor mainsprings. It is of large dimensions, a splendid piece of workmanship, very complete, and is now going in his manufactory, at Darlington, where it excites much attention."—*Chelmsford Chronicle*, May 2, 1845.

Thermo-Electricity.—Prof. Stefani obtained a thermo-electric current from the contact of hot with cold mercury. In a board, he made a canal a yard in length, and ½ an inch in depth and width; in the middle of the canal, he made a round hole 2 in. in diameter; and right and left of this he made two others, one at each end of the canal, and the others half way between; he placed the galvanometer wires in the extreme holes, together with two delicate thermometers; he then filled the five cavities with mercury, and poured mercury into the canal; but, by means of card diaphragms, he kept a little part of the canal free of mercury, so that the circuit was not completed; he now heated the mercury in the centre hole, and removing the card, the hot and cold mercury came into contact, and the galvanometer, which was one of short thick wire, was deflected from 5° to 8°.—*Il Cimento*, Ann. iii. 1845, p. 73.

Trombe at Escalquens.—On September 19, 1844, at 11½ a.m., a west wind came into conflict with a violent east-south-east wind; a dry trombe, in the form of a vast inverted cone, was manifested in incessant rapid rotation. Suddenly it seized upon a field of maize, and dispersed it in all directions; it moved onward from west to east nearly: trees were rooted up or torn and twisted. The gutter tiles from a farm-house were hurled to a distance with great noise: the entire róof of a shed was carried away; posts and rafters were transported to a distance of 600 or 700 metres; a wall of the shed was thrown down; a lattice-door was broken to atoms, and the pieces were not to be found; hay and straw were carried away; the latter was drawn into a rope: a bolt was torn off a closed door: an iron rod 1½ metre in length was torn from the dome of a dovecot, and carried 200 metres; the solderings of a cross at the middle of this rod, and of a zinc ball at its base, were partly melted: the

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dome, which was entirely iron or zinc, was displaced : all the poultry of the farm-yard were killed by the fall of the tiles, timber, &c. and perhaps also by electric discharges. A youth of 13 or 14 was carried up by the trombe ; he vainly tried to seize on the branches of the trees, around which he was being whirled ; he was thrown to the earth, and taken up again several times, without however sufferring any serious accident. A man of thirty years of age, rushed from the shed to escape the danger ; he was lifted up, and then was rolled upon the ground beneath the shower of tiles and timber. The trombe then passed onwards over the fields, destroyed a vine, and a field of maize, and carrying off the roofs of any houses in its course, it reached another farm about two miles from the former. It travelled by ricochets ; no rain fell at the scene of devastation ; but at Toulouse, six miles westward, thunder rolled, and abundance of hail fell, and such rain as had not been seen within the memory of man. These are the facts. The Abbé Chombron, who examined the place next morning, found that all who were witnesses of the trombe heard a continual dead and terrifying noise ; those who were in the midst of it saw fire, which does not appear to have been seen by those near, or by others at a distance ; the latter only noticed a kind of smoke. The fusion of the solderings of the dovecot convinced the Abbé that electricity was present in the meteor. His conviction is confirmed by the fact that the agents of the insurance company, *La France*, against their own interests, for the farm house was insured, were quite ready to admit it to be the effects of lightning. They also recognised traces of lightning elsewhere ; as for example, in the kitchen.—*Comptes Rendus*, Oct. 21, 1844.

Effect of a Storm.—The German journals state, that, in the neighbourhood of Darmstadt, during a storm, the electric fluid suddenly discharged, in the midst of a very violent noise of thunder, upon the lake of Wogsdamm, near the city, and, at the same instant, there appeared in the water, a small island, about thirty yards across in its widest part. On examination, it was found to adhere firmly to the bottom of the lake.—*L'Institut.* No 605. Vol. xiii. No. 31, Aug. 6, 1845.

Lightning Conductor.—At the meeting of the American Philosophical Society, on Sept. 20, 1844, " Mr. Smith drew the attention of the society to certain phenomena, presented by a house provided with a lightning-rod, and recently struck by lightning. The electrical fluid did not strike the rod, but communicated with it before reaching the ground. Mr. Smith considered that the rod was not sufficiently elevated above the house."—*Proceed. Amer. Phil. Soc.* No. 31, p. 102.

The Beech Tree, Non-Conductor of Lightning.—" So notorious is the fact," says an American paper, " that the Indians, whenever the sky wears the appearance of a thunder-storm, leave their pursuits, and take refuge under the nearest beech tree. In Tennessee,

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the people consider it a complete protection. Dr. Becton, in a letter to Dr. Mitchell, states that the beech tree is never known to be struck by atmospheric electricity, while other trees are often shattered into splinters."—*Athenæum*, Aug. 23, 1845.

Grotthus's Theory.—Certain phenomena prescribed by the gas battery have led Professor Grove to doubt the truth of molecular decompositions. "In a *single pair* of the gas battery, we must, according to the theory of Grotthus, suppose that oxygen and hydrogen tear asunder oxygen and hydrogen already united; for, whether we suppose the action to begin at the hydrogen extremity of the electrolyte, or at the oxygen, or at both simultaneously, the force which produces the composition of water is regarded by the theory sufficiently strong to overcome the force by which its constituents are already united, *i. e.*, a force is equal and unequal at the same time." Cases also occur of a feeble affinity overcoming a more powerful one, as when oxygen and deutoxide of nitrogen are the gases by which water is electrolyzed,--other cases occur in which chemical affinities are reversed, but there is no instance of this divellent and quiescent qualities being identical.—*Phil. Mag.*, Nov., 1845.

Electrical Amalgam.—"Good effects are never obtained with an electrical machine, without the cushions having been previously metallized. For this purpose, the common amalgam of zinc and tin, reduced to an impalpable powder, is employed. After the cushions have been warmed, a little grease is passed over their surface, and they are rubbed together so as to impregnate them equally and perfectly with the fatty matter. This being done, their surface is rubbed over some amalgam spread upon hot paper; and when they are nicely covered with the metal, they are rubbed strongly against each other, until they present a perfectly homogeneous and smooth metal surface. In this state, they are covered with mosaic gold reduced to powder, in the same manner as they are covered with amalgam. Philosophers, as well as the instrument makers, attach great importance to the choice of the mosaic gold, and it has happened to them, as it often has to me, without being able to explain it, to find bisulphuret of tin absolutely inert, to say nothing more. These variable effects must evidently be attributed to the impurity of the mosaic gold, because pure products must always lead to identical results. In fact, we are in the habit of employing the mosaic gold in lumps; in this case, in order not to alter the cushions, nor scratch the surface, it ought to be unctuous, and consequently a little heated; but it then contains, as I was able to satisfy myself of, hydrochlorate of ammonia, the hygrometric and conductric properties of which completely destroy the action that we are desirous of obtaining; I have found in mosaic gold, delivered by the best makers, as much as 50 per cent. of sal-ammoniac. Before employing the bisulphuret, it is necessary to deprive it of the foreign salts it contains. For this purpose, I reduce it to powder and wash it on a filter, until it no

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longer gives indications of the presence of sal-ammoniac; I then dry it, and employ it in powder upon heated paper. Since I have adopted this mode of purification, I have never had the least accident, nor the smallest interruption in the electric machines employed in my lectures or in my experiments.

"What is the part played by the mosaic gold in electrical apparatus? Notwithstanding the opinion of certain philosophers, who attribute to this substance an electro-chemical part, resulting from its decomposition by friction, we think that the mosaic gold undergoes a very slight alteration, that it is electrized by simple friction, and, moreover, that it is less adherent to the cushions, because the rubbed points then become more numerous. The lower metal surface, the importance of which we have recognised, would have the effect of discharging more promptly the electricity of the non-conducting particles of the mosaic gold."—M. Masson. *Archives de l'Electricité.* Sept., 1845.

Omnia.—The Cornwall Polytechnic Society offer a premium of ten pounds, "for the best practical method of obviating the corroding effects produced by solution of copper and other substances, in the *feed and injector water*, on the boiler and other parts of the steam engines used in Cornwall."—We regret to announce the death of M. Peltier, which occurred in Paris, in October.—A very efficient system of Cook and Wheatstone's electric telegraph is just completed on the South Eastern Railway.—A brilliant aurora was noticed on the night of December 3d; it was described by a resident at Nottingham. The *Editor* saw it between Croydon and Streatham.

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ELECTRICAL MAGAZINE.

April, 1846.

CXXVIII. *On the Electric Properties of Bodies in Combustion.* By Pierre Riess. Poggendorf Annal. t. 61, p. 545. Archives de l'Electricité. Vol. v. p. 91.

(translation.)

I. *Historical Notice.*

Toward the end of the sixteenth century, when electrical phenomena began to be studied, it was observed that bodies in combustion exercised a particular electrical effect. Gilbert* discovered that electrized bodies attract neither flame nor substances in combustion, and that they exercise no influence upon a needle placed in equilibrio at the extremity of a vertical rod, when it is in the vicinity of a flame of a taper or of any other substance in combustion.

Kircher revived this experiment with a little embellishment. "It is certain," said he,† "that electric bodies, placed in the presence of fire or glowing coals, or exposed to the rays of the sun, cease to exercise their action, as if these substances were the declared enemies of their energy.

In the memoirs of the *Accademia del Cimento*, published in 1667, is found an experiment that confirms in a decisive manner the curious property of flame in destroying the electricity of a body placed in its vicinity. If, it says,‡ after

* Gilbert's *Tractatus de Magnete.* London, 1660; book ii. c. 2. Later edition,* Sedini, 1628, p. 61. (The asterisk here and elsewhere indicates the edition I employed.)

† Kircher's *Magnes sive de Arte Magnetica.* Rome, 1641; book iii. part 3. Later edition, Rome,* 1651; p. 454.

‡ *Saggi di Naturali fatte nell' Accademia del Cimento.* Firenze, 1667. Later edition,* Naples, 1701; p. 229.

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having rubbed a piece of amber, it is moved about a flame, the latter takes from it its power of attraction; and it must be rubbed again in order that it may be restored. If it had retained at its surface a fragment of any substance, it lets it fall immediately on approaching a flame.

Another experiment connected with this is that related several years later by Otto de Guericke* without explanation. A down feather, when electrized with a ball of sulphur that had been rubbed, was not again attracted by it until after it had touched a foreign body. But if the feather with the ball were made to approach a lighted candle, the feather was attracted by the ball, when it was not more than a few inches from the flame.

Dufay† affirmed, in 1733, that a flame of an insulated candle could not be electrized when it was brought near to a glass tube that had been rubbed; and he attributed this fact to the parts of which the flame is constituted being continually renewed. He could not see in this the effect of heat, because red hot iron or a live coal, placed under the same circumstances became electric.

In 1744, Winkler observed the transmission of electricity by flame.§ An insulated lighted candle having been placed beside an electrized glass tube, another tube of sheet-iron, placed at the distance of an ell, became electric.

In the first memoir that received a prize from the Berlin Academy (May 31, 1745), Waitz‡ relates that a flame of a candle or a live coal, when placed under an electrized bar of iron, deprives it of its electricity. A cast-iron tube lost its electricity, as soon as a lighted candle was placed at two feet distance beneath it. The following experiment was made in order to demonstrate the property possessed by flame of transmitting to a conductor placed near it the electricity taken away from an electric body. A lighted candle was placed at each extremity of a long insulated band of wood, and an iron bar was suspended by silk cords above each candle; when one of the bars was electrized, the other became electric; but this effect ceased to occur when one of the candles was extinguished.

A short time afterwards similar experiments were made in France and in England. Du Tour wrote to the Abbé

* *Experimenta Nova Magdeburgica.** Amsterdam, 1672; p. 147. M. Gasp. Schott's *Technica Curiosa,** 1664. This experiment is not reported again.

† *Histoire de l'Académie des Sciences,** 1733; p. 84. Amsterdam,* 1737; p. 117.

‡ *Gedanken von d. Elektr.* Leipzig, 1774. Waitz, *Adhandl. von d. Elektr.** Vorbericht, p. 28.

§ Waitz, *Adhandl. von d. Elektr.** Berlin, 1745; p. 52.

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Nollet, Aug. 21, 1745, that he had discovered the property possessed by flame of destroying electricity;* Miles recognised the conductibility of smoke and flame;† Watson, on Oct. 30, 1746, announced to the Royal Society of London that a person placed on an insulating stool, transmitted the electricity with which he was charged, to another person insulated in a similar manner, when a column of smoke or flame was between them.‡

It was only in the years which followed that new facts were discovered. Franklin directed against an electrized body the smoke of fused rosin, and found that it did not conduct electricity.§ Jollabert‖ submitted a metal wire to the wick of an insulated candle that he had placed under an electrized bar of iron. The wire became electric when the candle was lighted.

Nollet¶ held, at 5 or 6 inches distance from a glass tube that had been rubbed, a bar of iron heated so as to give out sparks, and in three seconds the tube lost its electricity, whilst it still remained electric at the end of five seconds, when the bar had only been heated to dull redness. This same glass tube preserved its electricity when it was put in the focus of a burning glass of two feet in diameter exposed to the sun.

In 1766, Priestley made a more extensive series of experiments.** A charged Leyden jar lost its electricity when it was held two or three inches above or below the flame of a candle. A fire-shovel heated to redness, did not take away the electricty from the jar, unless held at a much less distance; and a glass-rod, heated to redness, did not take it away at all, even at a distance approaching to contact. The arc of communication of an electric battery was interrupted by a space, the extent of which might be varied at pleasure. When this space reached three inches, and a lighted candle was placed in it, the battery gave its discharge; when the candle was replaced by a piece of red hot iron, it was necessary in order to obtain the discharge, to reduce the extent of the space, when the circuit was interrupted to 1½ inch; finally, with a rod of heated glass, the interruption could not be greater than just sufficient for the extremities of the

* Priestley, *Geschichte der Electric.*

† De Humboldt, *Gereizte Muskel und Nervenfaser,** vol. i. p. 438.

‡ Priestley, *Geschichte,** p. 52.

§ *Experiments and Observations*, 5th ed.* 1774; p. 6.

‖ *Recherche sur l'Electricité.* Paris, 1749. *Mémoir. de Math. présentés à l'Acad.** 1755; vol. ii. p. 251.

¶ Nollet, *Recherches sur la Cause de l'Electricité.** 1753; p. 216.

** Priestley, *History of Electricity.** 1767; p. 612.

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connecting wire not to touch the glass rod. To study the passage of the electricity through smoke and vapour, Priestley increased by only a small quantity the extent of the interrupted part of the circuit, so as to be able to obtain the discharge through atmospheric air. Vapour of water projected from the spout of a vessel in a state of ebullition, did not determine the discharge; the same was the case with the smoke of resin in fusion, or that of a candle which had just been blown out. But when the wick of the candle, while still in a state of ignition, was placed in the interrupted part of the circuit, the discharge of the battery took place, and the candle was lighted again.

In the preceding experiments one important feature in the phenomena has not attracted attention. Indeed, whilst the flame destroys the electricity of a body placed in its vicinity, it becomes itself charged with electricity of the same kind as that of this body. Thus, on observing, under these circumstances, the electricity of flame, the electricity of the body to which it is brought near may be known, and this frequently when it cannot be studied directly. For example; if a rubbed resinous surface be held at such a distance above an electroscope that the latter is not affected; and if a flame or an ignited body be put in communication with the coating of the instrument, the instrument immediately gives signs of resinous electricity. Consequently the body in a state of combustion furnishes a means of increasing without limit the sensibility of an electroscope. This highly important application was made in 1787, by Bennet,* and by Volta,† and was employed to study atmospheric electricity and the electricity developed in evaporation.

Alexander de Humboldt‡ studied the electric action of different flames, and found the same with the flame of an oil-wick, a taper, phosphorus, hydrogen, and sulphur.

Bonnycastle§ found that the flame of sulphur does not exercise any action, unless when it is very intense and almost of a white colour; but that it does not act at all when it is blue, and arises from a combustion of a small part of the sulphur.

* *Philosophical Trans.* § 1787.* p. 288. The memoir was read on May 10th of the same year.

† Volta, *Lettres Météorologiques* (Schœffer's German translation).* 1793; p. 85 and 137. The letter relating to the subject in question was addressed to Lichtenberg, in July 1787.

‡ De Humboldt, *Versuche über d. Gereizt. Muskel u. Nervenfaser.** 1797; vol. i. p. 440.

§ *Quarterly Journal*, 1829. Schweigger's *Jahrbuch d. Chemie,** vol. lviii. p. 194.

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Among the explanations that have been attempted in order to account for this phenomenon, I at once set aside those in which authors have had recourse to causes that they have devised specially with this object in view. Waitz* put forth the idea that electric matter differs very little from igneous matter; Bonnycastle supposed that flame lends a radiating force to the electricity that traverses it; and recently Petrina has conjectured that the oxygen which arrives under the form of a current upon the flame, does not form the chemical combination, from which this flame is derived, except when it is found in a determined electrical state; and that it takes and preserves this state to a considerable distance from the point where the combination takes place.

Among philosophers, by far the greater number have admitted that flame acts only in a secondary manner in the electric phenomena in question, and that it is the current of air and vapour produced by the heat of the flame, which is the immediate cause of these phenomena. As far back as 1747, Du Tour attributed the action of flame to the volatile parts that escaped from it; according to him these parts become charged with the electricity of a neighbouring body and carry it away with them.† But the demonstration that he desired to give in support of this opinion, by placing between the flame and the electrized body screens of paper or glass, and thus destroying the effect of the flame is not sufficient. This philosopher was not more fortunate in another memoir;‡ he only examined the case in which flame exercises its influence over a body placed directly above it; and he shows that a jet of water, employed in place of the current of vapours, produces the same effect as the latter. The experiment in which the column of smoke from an extinguished candle exercises the same action as flame, is not decisive, as far as concerns the hypothesis in question; since the effect of the smoke continues only so long as the wick continues to be in ignition.

The hypothesis in question finds a better support in Priestley's experiments, which I have related above. Indeed, they show that the air, which surrounds a body in combustion, and not only in continuous ignition, becomes a conductor, and consequently serves to determine the discharge of a battery through a space, where it would not have occurred, if only ordinary air had been found there.

* (?) Poggendorff's *Annalen der Phys. und Chem.** vol. lvi. p. 463.
† *Mémoires présentés à l'Acad. des Sci.** t. i. p. 345.
‡ Ibid,* t. ii. p. 246.

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Volta has treated the action of flame, with the attention that he was in the habit of giving to all subjects, that he took up; and he consecrated to it a great portion of his meteorological letters. He takes as a starting point in his hypothesis, the fact that a metal point, brought near to a body highly electrized, takes away its electricity, in the same manner as flame does; and it becomes itself electric. In this case, according to his view, it is the current of air produced around the point by the electricity itself, which splits this point, at the same moment in contact with a greater number of particles of air, than if the air were not in motion; and which, by this means, makes it acquire in a short time the electricity of the body placed near it; or else makes it yield up its own electricity to that body. If the electricity of the latter is too feeble, or the interval between it and the point too considerable, the current of electric air is not established, and the point does not exercise its action. In which case, the point merely receives from the body that is brought near to it, induced electricity (*accidental*, as Volta terms it), which disappears at the same time that the body is withdrawn.* Now this effect, that the current of electric air produces with the point under favourable circumstances, constantly occurs with the flame, by the effect of the violent current of air, always ascending, and which results from the existence of the flame itself.

We must first remark that it is now completely impossible to admit this hypothesis. The current of electric air is not the cause but the consequence of the strongly electric state of a point, and the most powerful current of air, that can be determined by a pair of bellows against an electrized body, will not take away its electricity sooner than the same air at rest would have done.

The other circumstance which, according to Volta, determines the action of flame, is that the air becomes a conductor by the effect of its refraction and heat; and that hot air, being lighter, takes and abandons electricity more easily than cold air. Consequently a conducting zone originates from the flame, which is much more extended than the visible part of the flame itself, which proves that the flame might be shut up in a lantern without its being made to lose its action, provided that a sufficient current of air were furnished to it.

This last argument is identical with that of Priestley's; only, according to the English philosopher, the conducting zone is filled with the emanations of the flame, whilst, ac-

* *Meteorological Letters*, &c. p. 151.

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cording to Volta, it is the hot air which so especially possesses conductibility. But this hypothesis has not been found to be true. Read* placed in a heated furnace two simple electroscopes, charged with electricity; although the heat was so strong that one of the electroscopes could not resist it, the other preserved its electricity, when it was drawn out. This experiment, when repeated by Hermann,† in a wind furnace, gave the same result.

Alex. de Humboldt‡ attributes the action of flame to the conductibility of the whole of the substances that emanate from it, substances which, according to the nature of the flame are composed of carbonaceous particles, mechanically drawn from the body in combustion,—vapour of water, carbonic acid, vapour of oil, imperfect sulphuric acid, phosphoric and other more complicated acids, enveloped in an atmosphere, that is rarified, and in a great measure deprived of its oxygen.

II. *Flame exercises its action by the particles of vapours that acts as points.*

Heretofore all hypotheses that rest upon experiments to explain the electric action of flame, tend to suppose that there exists between the flame and the electric body, that is brought near to it, an invisible communication by means of which an actual transmission of electricity occurs between them. But this hypothesis can serve to explain only a small number of the entirely special experiments, and leaves without solution the questions relating to the most important effects of flame. I placed an electroscope in the open air under a clear sky, and I armed it with a conductor two feet in length. For an hour, I perceived no divergence in the leaves of the instrument; but a divergence immediately occurred, when I removed the conductor, and brought near to the conductor a flame of spirit of wine or a piece of lighted tinder. One cannot conceive how the vapour of the flame and of the tinder can in an instant enter into contact with a greater number of electrized particles of air than the conductor of two feet in length did in one entire hour. I fixed in a horizontal position an insulated disc of metal, at eight inches above a spirit of wine flame burning in a metal lamp; and I placed it in connection with a gold-leaf Bennet's electroscope. When the lamp was electrized, the electroscope presented a divergence of three lines, whilst another electro-

* *Summary View of the Electricity of the Earth.* Lond. 1793. Saxtorph, *Electricitæslehre;* Fongel's* German Translation. 1803; part i. p. 222.

† *Abhandl. del Akad. der Wissench.* Berlin, 1819; 2nd part.

‡ *Versuche über die ger. Musk. u. Nervenf.** part i. p. 439.

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scope put into communication with the lamp, gave a divergence of only two lines. If the disc were electrized, the divergence of the electroscope, that communicated with the lamp, was raised to four lines, and then diminished, whilst in the electroscope that communicated with the disc, the divergence rapidly rose to five lines, and then became nothing. But at each experiment the *two* electroscopes no longer gave divergence; as soon as one or other was placed in connection with the ground. In this case there was evidently between the lamp and metal disc a communication established by means of the vapour. But the experiment may be made in another manner still. A copper disc 3 in. 11 lines in diameter is fixed to a stem of gum-lac, in a vertical position and in the neighbourhood of a metal spirit-lamp, 9 lines high, so that the right line drawn from the extremity of the flame to the centre of the disc is perpendicular to the latter, and consequently horizontal, and about 3¾ in. in length. The lamp was enveloped in a copper tube about 13 lines high and 9 lines in diameter; the copper disc communicated with an electroscope, whose gold leaves were about an inch in length. When the lamp was lighted, and was electrized by putting it in contact with the pole of a dry pile, the electroscope immediately gave a divergence of 3 lines, which was reduced to 2½ when the lamp was connected by means of a wire with the gas tubes of the house. But this divergence was not sensibly altered by the flame, and it was still 2 entire lines at the end of two minutes. When the copper disc was electrized, and the lamp was again put in complete communication with the ground, the leaves of the electroscope slowly approached; but remained, however, at 3 lines distance; and at the end of two minutes they diverged again to 2½ lines. If the lamp had not been lighted, the divergence of the electroscope fell from 13 to 12 lines, when the lamp was put into connection with one of the poles of the dry pile. I repeated these experiments several times; and they always gave me the same result. They are contrary to the hypothesis, unreasonable indeed in itself, that the stratum of air which separates the flame and the copper disc, completely screened against it, was in the instant entirely filled with conducting vapours by a flame of scarcely 4 lines in height. The flame is situated at one of the limits of this zone of air, and the electroscope at the other; now the latter does not lose its electrical state by the fact of the flame being in complete communication with the soil, as ought to be the case even where this space is only occupied by a body that is a partial

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conductor. The fact that flame diminishes to a certain degree the electricity of the electroscope, and brings to it an electricity of only a certain degree of force, agrees perfectly with the following hypothesis, which explains, in a more probable way than any other, the action of flame, and the accuracy of which is established by experiments, that I will relate in the sequel.

A dense current of vapour constantly escapes from flame, which rises into the air in the form of a continuous column. But this current retains this form only to a very inconsiderable height. The air penetrates from all sides into this column; the vapour, experiencing a powerful expansion by the effect of the great heat, mixes with the particles of air; the mass is thus cast up and divided into a great number of points, and nothing remains but filaments which go on alternating themselves more and more, and are finally dissipated in the air. From the mass, therefore, of vapour both within and above the flame, there come forth filaments that are also conductors, and separated from each other by the non-conducting gases that are formed, and by hot air, and are necessarily terminated in points. Under this supposition, flame must be considered as a good conductor of electricity, furnished with numerous points, penetrating at all sides into the atmosphere, and surpassing in fineness all those presented by nature. If all the experiments, that have been made with imperfect points, are repeated with this conductor, we have no difficulty, by aid of the known properties of electricity, in rendering evident the electrical effects that are due to flame. The following is a description of the most complicated cases.

From a conductor furnished with points there escapes a quantity of the electricity it has received, which is greater in proportion as its points are finer; from flame also, the greatest proportion escapes. At the extremity of a point the electric density is much greater than in every other spot of the conductor; and it may be demonstrated that this density is very considerable at the extremity of the points of the vapour of flame. In fact, if a non-electrized conductor is brought near to an electrized flame, taking care to hold it beyond the filament of vapour, this conductor is electrized by induction, its points that are nearest to the flame acquiring contrary electricity. (I will name it induced electricity of the *first* kind, to distinguish it from electricity communicated to the flame.) The density of the electrical stratum of these nearer points of the flame, depends upon the distance, by which they are separated from the points of

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vapour, and on the electrical density of these latter. But, even when they are at a great distance, these points are so strongly electrized, that the electricity accumulated there escapes, and the conductor then preserves the induced electricity, which I call of the *second* kind, which is of the same nature as that of the point of vapour. It would seem, from this complex result, that the electricity communicated to the flame has really passed to the conductor, that has been brought near to it. If a conductor charged with electricity is brought near to a flame, the points of the vapour of the flame are electrized by induction; but they allow the electricity that they receive to escape without interruption, and there remains in the flame induced electricity of the *second* kind, which is similar to that of the conductor. But, at the same time, the electrized points of vapour exercise the same action as in the preceding case, and this action has the same result as if the electricity of the points had passed to the conductor. These two electricities not being similar, the body, that is brought near, continually loses its own. But this loss is not without limit. Indeed, as the energy of the electrization of the point of vapour depends upon the electric density of the body that is brought near, the action of the points must cease when the density descends below a certain limit. Thus the flame only takes away a portion of the electricity from the body that is brought near to it, so long as the points of vapour do not really reach this body, and only act upon it by induction. It would be a wearisome task to follow out further the study of the phenomena of flame; moreover, this would be superfluous, because they may all be deduced with the same facility in the same manner.

If a metallic point is surrounded by a conducting tube open at one of its extremities, its electric density diminishes, and may be reduced to zero, according to the dimensions of the tube. A point of a fine needle 3½ lines in length, and fixed on a disc of metal, was enclosed in a cylinder 13 lines in height, and 9 lines in diameter. However strong the electric charge of the disc were, the point presented no electricity, and consequently exercised no action upon a conductor that was brought near to it. The same means, when employed to destroy the action of the points of the vapour of flame, did not succeed. I repeated the experiment described above, with this difference, that the spirit-lamp burned in an iron tube 27 lines high and 9½ in diameter, and I obtained electricity: however the disc, when placed in a vertical position, became charged with electricity, although in a

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much less quantity than formerly. The cause is evident: the tube concentrates the ascending current of vapour,—it forms, at its opening, points of vapour, which exercise on the vertical disc the same action as before, only at a greater distance. The flame that burns in the iron tube loses its electricity as completely as that which burns in the open air; and if a conductor is fixed above the tube, it becomes as electric as it does when the tube is removed. These experiments, the results of what might be anticipated, acquire some importance by the facts which are contained in the sequel.

III. *Electric property of Bodies in Ignition.*

Bodies that burn with flame are not the only bodies which possess in so high a degree the property that characterizes points; we must add to these bodies that burn without flame, such as German tinder, wicks, pastiles. The effect of these latter is not produced by vapour, but by the points which are formed on the very surface of the body during combustion. At the spot. where the mass of carbon is consumed, for example, there is formed a cavity, the edge of which is protected against combustion by the carbonic acid produced in this phenomenon. At the spot where several of these closely packed cavities meet together, they leave between them a projection, formed by the substance that the fire has not destroyed, whence it follows that a plane surface acquires a rough aspect during combustion. But after the carbonic acid is liberated by the effect of the heat, and a new call for oxygen results, each of the little asperities begins to burn from below upwards, and thus gradually disappears, until nothing more than a point remains, which is destroyed in its turn, and which, after the manner in which bodies in combustion behave, under electric relations, may be regarded as very nearly perfect. Indeed, all that we have said above, in respect to points of vapour, may be applied to these latter, with this single difference, however, that the points of vapour constantly change place in the air, whilst the points of carbon remain at the surface of the body in combustion, and are there incessantly replaced by new ones. All the electrical effects of a body in ignition are explained perfectly well by this hypothesis, the accuracy of which may otherwise be demonstrated in a decisive manner. Indeed, if it is not to the state of ignition, nor to the vapour which rises, but rather to the presence of a point that the effects in question are due, one ought also to be able to make them disappear in the same manner as for an artificial point. Now we succeed per-

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fectly in this. To stems of gum-lac I fixed in a horizontal position, and at the distance of 5½ in. above each other, two upper discs 3 in. 11 lines in diameter, and I made the upper disc communicate with an electroscope, and the lower with the pole of a dry pile, the other pole of which had been placed in connection with the ground. When a piece of lighted tinder, about an inch in length, was placed upon the former, the electroscope immediately diverged 5 or 6 lines; but not the slightest divergence was perceived when the tinder was included within a copper cylinder 13 lines high by 9 wide, although the smoke, rising under the form of a thick column, struck against the upper disc, as in the former case.* When the tube is taken away the divergence reappeared, and thus its influence might alternately be applied to and taken away from the tinder. In the same manner, what is called the force of absorption of this substance when in ignition may be made to cease. The upper disc was placed in connection with the dry pile, the lower with the electroscopes, and lighted tinder was placed on the latter. If the tinder were free, the electroscope immediately diverged; if it were imprisoned within the cylinder, no indication of divergence was manifested. Finally, when the lower disc, in connection with the electroscope, was electrized by means of a momentary contact with the pole of a dry pile, so as to determine a divergence of 13 lines, and lighted tinder was then placed upon the disc; this divergence very rapidly disappeared; but it endured for a certain time, when the copper cylinder was placed upon the tinder.

It clearly results, from all these experiments, that the vapour of lighted tinder does not conduct electricity, and that the electrical effects which are observed are due to the points formed upon the surface of the tinder, in consequence of its combustion. The same results may be obtained with a cotton wick; but with pastiles the effects are not so positive, because their smoke is a feeble conductor of electricity when the combustion is active. In order to escape from the inconvenience arising from the smoke, I had some candles made an inch in length, of powdered charcoal and a small proportion of saltpetre, mixed with gum tragacanth. These candles gave no smoke; and in order that the vapour, which was formed, might not interfere with the experiment,

* It is scarcely necessary to say, that at each experiment, we must begin by discharging the electroscope, by touching it with the finger; otherwise, instead of not obtaining any divergence, there would be one (of half a line or more) arising from the induction of the metal bodies, even in the case where bodies in ignition were not employed.

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I let the combustion take place from below upwards. With this view, I fixed the candle on the upper of the two 5-inch copper discs, that were placed one above the other, and I directed its point toward the centre of the lower disc. The two discs were each separately in connection with the electroscope; the upper disc was electrized, and the electroscope was observed at intervals of half a minute. The following are the results that were obtained:—

	Interval of Time.	Upper Electroscope with Candle.	Lower Electroscope.
Candle burning in the open air .	0 min.	12 lines	3½ lines.
	½ „	3 „	2¼ „
	1 „	1 „	2¼ „
	(4?) „	(discharge)	2¼ „
New candle enclosed in a cylinder 13 lines high, and 9 in diameter .	0 min.	12½ lines	0 lines.
	½ „	11½ „	0 „
	1 „	10½ „	0 „
	4 „	8½ „	0 „

Thus, whilst the candle, when it was burning without flame in the open air, lost in one minute almost all the electricity that it had received; it possessed the greater portion at the end of four minutes, when it was enclosed in a conducting envelope. In the former case, it electrized the disc placed near it, and the latter preserved its electricity when the electricity was withdrawn from the candle, by means of a conductor; in the latter case, the disc acquired no electricity. The same results were always obtained every time these experiments were repeated. Afterwards, I changed also the position of the charcoal candle, and I fixed it in a horizontal position in a disc placed edgeways, placing opposite to it, at five inches distance, the other disc, also edgeways. The following are the results obtained by an experiment similar to the preceding:—

	Interval of Time.	Electroscope No. 1 with Candle.	Electroscope No. 2.
Free candle . .	0 min.	12 lines	0 lines.
	½ „	¾ „	2 „
Candle in cylinder	0 „	12 „	— „
	½ „	11½ „	0 „
	1 „	10½ „	0 „
	2 „	9 „	0 „

The influence of the envelope is also manifested in this experiment. The electrization of the electroscope No. 2, is not considerable, because it occurs in an interval of time,

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during which the candle, momentarily electrized, is uninterruptedly losing its electricity. If we desire to have stronger indications of electricity, we have merely, without giving the charcoal a more powerful electric charge, to electrize it in a more constant manner for a longer space of time. When the charcoal candle has remained in communication with the knob of the dry pile, the electroscope No. 2, of the disc placed opposite, gave a divergence of five lines at the end of a few seconds; but it did not give any even after the longest interval of time, when the candle was placed in its envelope.

Hitherto, in these experiments, whose arrangement I have varied simply on account of their importance, the loss of electricity from a candle burning without flame has been observed, as also the electrization of a conductor placed at a little distance. In the experiment that follows, I endeavoured on the contrary, to study the electrization of the charcoal, and the loss of electricity of the electrized conductor.

	Interval of Time.	Electroscope No. 2.	Electroscope No. 2 with Candle.
Free candle	0 min.	12 lines	$3\frac{1}{2}$ lines.
	$\frac{1}{2}$ „	$11\frac{1}{4}$ „	$3\frac{1}{2}$ „
	1 „	11 „	$3\frac{1}{2}$ „
	$1\frac{1}{2}$ „	$10\frac{1}{2}$ „	$3\frac{1}{2}$ „
	2 „	$10\frac{1}{3}$ „	$3\frac{1}{2}$ „
	3 „	10 „	$3\frac{1}{2}$ „
		discharge	$3\frac{1}{2}$ „
Candle in cylinder	0 „	12 lines	0 „
	$\frac{1}{2}$ „	$11\frac{1}{3}$ „	0 „
	1 „	„ „	0 „
	2 „	11 „	0 „
	3 „	$10\frac{3}{4}$ „	0 „
Free candle			3 „

Whilst the charcoal candle burning in the open air acquired a quantity of electricity capable of producing a divergence of $3\frac{1}{2}$ lines, it acquired absolutely no electricity when it burned in its envelope. The influence exercised by the charcoal upon the loss of the electricity of the nearer disc was feeble, as might be expected. Indeed, this is because a charcoal candle, electrized so as to make the electroscope diverge $3\frac{1}{2}$ lines, can only feebly electrize a disc, placed at the distance indicated; it is a similar case to that presented in the experiment of the action of flame. I must, in passing, recall to mind, that these latter experi-

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ments, as well as all the preceding, are entirely contrary to the opinion hitherto admitted of the passage of electricity from an electrical body to a body in combustion, since the loss of electricity experienced by the former, presents but a feeble difference, whether the charcoal candle placed in front, has or has not received from it a considerable quantity of electricity.

The following are a few experiments that I will mention as an appendix. Every body in combustion does not exercise an electric action by means of points of vapour or solid points; for this, it is necessary that the vapour, which emanates from it, should conduct electricity, or that the substance of the body permits of the formation of points. Sulphur may serve as an example. The vapour of sulphur and sulphurous acid, insulate electricity; and, as sulphur becomes liquid before burning, points cannot be formed at its surface. Burning sulphur in a mass does not, therefore, possess the electric properties of bodies in combustion. I placed above each other, in a horizontal position, two copper discs; and upon the lower disc I placed a copper capsule filled with sulphur, the bottom of which had a surface of a square inch. When this capsule was entirely filled with liquid sulphur in combustion, the upper disc placed at the distance of two inches, preserved all the electricity that had been transmitted to it. But if a drop of sulphur in combustion were taken away with a rod of glass, this drop, held even at the distance of five inches below the disc, took away from it all its electricity. There could be but one cause for this, namely, that the vapour had become a conductor in consequence of the free access of air (it was transformed into sulphuric acid?), as is proved by the fact that a tube of sheet iron three inches in length, beneath which the sulphur flame was burning, presented no change in its action.

Davy's lamp furnishes us with another example. It burns without flame; but the vapour that abundantly escapes from it is a very good conductor of electricity, and exercises precisely the same action as a conducting flame. On a horizontal copper disc was placed a similar lamp, the platinum helix of which was clear red-white throughout its extent; beside this disc, a second disc was placed in a vertical position at two inches and two lines distance from the helix. The lamp was contained in a copper cylinder. If electricity were communicated to one of the discs, the other immediately became electrical, and remained in this state even when the electricity of the former was taken away. As the

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lamp, to the upper turn of the platinum helix, was 10 lines in height, as the cylinder in which it was placed was 13 lines in height, and as the highest point of the copper disc, placed at the distance of 2 inches and 2 lines, was 1 inch 10 lines above the base of the lamp, it follows that, from not one of the points of the platinum helix, heated to glowing red, could be drawn through the air a right line that would fall upon the disc. The electric action of the lamp, therefore, is not due to the glowing red platinum, which will be curiously confirmed in the sequel.

In a great number of flames, the action of points of vapour, and that of carbon points, occur after each other. For example, a lighted taper exercises its action by points of vapour; if it is suddenly extinguished, the vapour preserves for some seconds its conductibility and its action; it then loses this property, and the wick, abundantly moistened by melted wax, remains without action, so that the smoking taper exercises no influence over an electrized body brought near to it. When the wax has ceased to rise in the wick, the latter gradually ceases to burn, and the carbon points formed at its surface, acquire an electric activity. We may thus explain numerous contradictions relative to the conductibility of the smoke of an extinguished taper.

IV. *Disturbances in the Electric Action of Bodies burning with or without Flame.*

In all the preceding experiments, the combustion of bodies has been maintained as complete as possible. To make it active, and at the same time to drive away the ashes, the tinder and the carbon candle have frequently been blown; and the spirit lamp and Davy's lamp have not been made use of, except when the combustion was intense. With the exception of this precaution, all disturbance in the effects that have been mentioned has been avoided. But this is not the case when the combustion of the bodies subjected to experiment, had not been brought about in a perfect manner. The modification that then occurred in the experiments, far from throwing any doubt upon the principle, relative to the effects produced, receives its explanation, and consequently serves to support it. This modification consists in this: I have not, in every experiment, pointed out which of the two electricities were employed in making it, because it succeeded as well with one as with the other. But if the combustion is moderated, we perceive that certain experiments succeed with much more facility and precision, when one of the

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electricities rather than the other is employed. The carbon candles furnish an example by which this is easily produced. A carbon candle, that had been lighted and covered with ashes to more than half its height, was attached to one of two discs, placed vertically opposite to each other, at 5 inches distance. When the positive pole of a dry pile was joined to this disc, the electroscope connected with the other disc slowly diverged, and at the utmost to 2 lines; when, on the contrary, the negative pole was made use of, the electroscope rapidly diverged to more than 5 lines. The following is a converse case: the electroscope, in connection with the candle, diverged more rapidly and more violently when the disc placed opposite to it had received positive, than when it had received negative electricity. It seemed, so to speak, that negative electricity escaped more easily from the candle than positive, and that the latter entered more easily than negative. Moreover, in this experiment also, the candle ceased to exercise any further action when it was enveloped in the cylinder.

In order to explain this singular phenomenon, we must call to mind that a development of electricity occurs during the combustion of the carbon, and that the carbonic acid that is disengaged possesses positive, whilst the charcoal gives negative electricity.* The points that are formed of the carbon in ignition have already originally negative electricity, and consequently ought to become more strongly electrical, and to exercise a more energetic action when negative electricity is given them, than when positive electricity is transmitted to them. This explanation agrees so well with the results of the same kind that preceded, that we are not to be astonished at seeing the effect in question produced, but rather that we do not observe it more frequently. In this respect, experiment teaches us that the particular electricity of lighted carbon becomes more strongly sensible when the carbon burns moderately ;† Volta was not able to render this electricity evident, except by the assistance of a free current of air, and by designedly retarding the combustion of the carbon.

Carbon exercises its action by its points, charged with negative electricity, and not by its vapours charged with

* I could easily convince myself of this by placing a lighted candle upon the screwed plate of Behren's or Fechner's electroscope, or by holding it under a metal gauze receiver, communicating with the branch of the electroscope ; in the former case, the instrument immediately gave signs of negative, and in the latter of positive electricity.

† Vide *Lettres Météorologiques* (German Trans.)* i. p. 200.

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positive electricity; if the contrary occurred, this action, in conformity with the explanation that has been given above, ought to exercise itself precisely in the converse manner for the two electricities. It would then be seen that positive electricity escapes more easily than negative from the body in combustion, and that negative electricity penetrates it more easily than positive. This case is presented in nature. There exists a species of combustion, in which points are not formed upon the body in ignition, and in which the electric action can only originate in points of vapour strongly charged with positive electricity. This occurs in Davy's lamp; and we know the singular electrical effects that are produced; Mr. Erman discovered them many years ago, and made them the subject of a special memoir.* I placed a brass lamp, the platinum helix of which was feebly heated to redness, beside a copper disc placed edgeways, so that the helix was 2 in. and 2 lines from the disc. The lamp, 10 lines in height, was enclosed in a copper cylinder 13 lines in height, and I satisfied myself that the helix could not be seen from any point of the vertical disc. When the lamp was charged with positive electricity, the electroscope in connection with the disc immediately diverged, and preserved the divergence even after the lamp had been put in complete communication with the soil; when the lamp was charged with negative electricity, the instrument presented only a slight divergence. When, on the contrary, the vertical disc was charged with negative electricity, the electroscope, when put in connection with the lamp, diverged at the very instant; but it presented only a slight agitation when the disc was charged with positive electricity. We thus see the same phenomena produced as had been observed with the carbon candle, only, in the two cases, it is the contrary electricity that produces the same effect.

In order to study the electric liberation peculiar to the lamp, it was placed on the plate of a Behren or Fechner's electroscope, the polar plates of which were 11 lines apart. Negative electricity was immediately manifested, and with such energy that it was necessary to remove it from time to time to prevent the gold leaf from striking the polar plate. But this energetic liberation of electricity only occurs when the helix is moderately red, and is only so in its upper convolutions; when it is heated to whitish red in its whole extent, a very feeble or scarcely any indication of electricity

* On a particular reciprocal action of the two contrary electrical principles. (Feb. 11, 1819.) Vide *Mémoire de l'Acad. des Sciences de Berlin*, 1819.

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was obtained. Mr. Erman has expressly* remarked, that the particular phenomena of the lamp are only produced, when the platinum helix is heated to redness in the upper convolutions.

The explanation of these phenomena has been already shown above. It is the points of vapour that produce the electric action in Davy's lamp; and, as these points possess positive electricity of peculiar energy, their action is incomparably more powerful when electricity of the same nature is communicated to them, than when negative electricity is transmitted to them.—[A.]

CXXXIX. *On new Relations between Electricity, Light, and Magnetism.* (Extract of a Letter from Mr. FARADAY to M. Dumas.) "Comptes Rendus," Jan. 19, 1846. Vol. xxii. No. 3. p. 113.

(TRANSLATION.)

"IF the *line of magnetic force*, engendered by a powerful electro-magnet or by a helix, is made to pass through a transparent body parallel to a ray of polarized light, traversing the same body, the ray of polarized light will experience a rotation. This effect is produced in all transparent liquid or solid bodies not endowed with double refraction, but in different degrees according to the nature of the substances. In this I perceive a magnetic action operating upon the luminous ray itself; but many of my friends, who however, have not yet been enabled to take into consideration all the facts of my memoir, are of opinion that this phenomenon proves nothing of the kind. Thus, although my opinion remains the same, I am willing to admit that it may be erroneous.

"If the luminous ray experiences rotation to the right for a certain given direction of the magnetic force, or of the current in the helix, it will experience it to the left for the contrary direction of the magnetic or electric forces. The direction of the rotation essentially depends upon the direction of these forces, which constitutes the extraordinary difference that I am about to disclose between this rotation and that determined by quartz, sugar, oil of turpentine, &c.

"Place side by side a certain quantity of water in a helix and a tube containing oil of turpentine. If the oil possesses right hand rotation, pass an electric current through a helix, so as to give rotation to the right, the water in the tube

* *Mémoires de l'Acad. des Sciences de Berlin*, 1819, p. 12.

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will acquire rotatory power to the right, and the two liquids will possess the same mode of action.

"Leaving now the tubes, the helix, and the current, in the state that we have just described, pass the polarized ray in the contrary direction through the tubes, and place ourselves for observation at the opposite extremity of the latter, we shall still see *the oil of turpentine turn the ray toward the right, but it will not be the same with the* WATER; it will turn the ray to the left. The rotation is absolutely connected with the direction of the electric current, which moves in the circuit, and which, seen through this extremity, passes to the left.

"If, instead of water, oil of turpentine had been in the helix, and the electric current were sufficiently intense to produce on the luminous ray a rotation equal to that determined by the oil, its rotatory power, observed on a ray passing in a certain direction, will appear double, while, examined by a ray passing in the contrary direction, it will be reduced to zero.

"It is upon this fact that I chiefly support my own opinion against that of my friends, or rather of some amongst them.

"The following are my results on the magnetic condition of matter. I find that all matter under the solid or liquid form (perhaps even under the gaseous form) is effected by the magnet, but not as iron would be. A magnetic substance such as iron is attracted by the magnet, and a portion of the elongated form of such substance places itself in the direction of the lines of magnetic force; but a substance which is not, like iron, magnetic, is repelled by the magnet, and an elongated portion of such substance takes the direction transverse to these lines of magnetic force. Water, alcohol, ether, oil, wood, flesh, blood, and a thousand other substances possess this magnetic relation; but the best are, perhaps, heavy glass, phosphorus, antimony, and bismuth. Perhaps you may remember that (about, I think, thirty years ago) M. Lebaillif, of Paris, showed the repulsion of a magnet by antimony and bismuth. I have mentioned this fact in a general manner; and I have cited it in my memoir.

"Having named the substances that are not magnetic after the fashion of iron, *diamagnetics*, I have preserved this name to express that new magnetic condition; and, to resume, I may say that every liquid or solid substance exercises and undergoes a magnetic influence, being in its nature either magnetic or diamagnetic.

"From this property, and from the study of it, arise a

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multitude of curious conditions, for which I am obliged to refer you to my memoir, which I will send to you as soon as possible.

"Among other things, I have proved that all the ordinary compounds of magnetic metals are in like manner magnetic. Thus, it is not merely the oxides of iron that are magnetic, as M. Becquérel and other observers have noticed, but all the salts of iron are equally so, and all the solutions of these salts, of a degree of concentration sufficient to counterbalance the diamagnetic force of the water or alcohol employed as solvents.

"By proceeding in this manner, I have been able to prove that cerium is a magnetic metal; for all its salts are magnetic; and it is the same with chromium and manganese."—[A.]

CXL. *Note on Mr. Faraday's New Experiments.* By M. Pouillet. "Comptes Rendus," Jan. 26, 1846. Vol. xxii. p. 133, No. 4.

(Translation.)

For several months, the subject of conversation has been a new series of experiments by Mr. Faraday, the object of which is one of the most important discoveries; that of the action of magnetism upon light. Two authentic documents upon this subject have at last reached us; one is published in the last number of *The Philosophical Magazine*,* as an extract from the meeting of the Royal Society on November 27th; the other has been communicated to us by M. Dumas, at our last sitting,† in the name of Mr. Faraday himself. Very diverse results are announced in these two publications; but a single fact is presented in them with certain developments,—it is that which relates to the action exercised by an electro-magnet upon a ray of polarized light, to make its plane of polarization turn either to the right or to the left, according to the relative directions of the resultant of the magnetic actions.

This fact is justly regarded by Mr. Faraday as a fundamental fact; for, up to the present time, there is nothing in science analogous to it; and in itself it constitutes a discovery of the highest interest.

All philosophers have undoubtedly hastened to repeat it; and to study it, in order first to recognise its perfect accuracy, and then to search out its most striking characteristics, and

* Vide *infra*. † Vide *supra*, p. 259.

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its most essential conditions. After having read *The Philosophical Magazine*, I immediately set myself to work, as I mentioned to the Academy at the last sitting; but these first essays having been without results, and other philosophers not having been more fortunate than myself in this enterprise, I deemed it necessary to return to it with more attention, varying the mode of experiment, and supplying, to the best of my ability, the very indefinite and incomplete indications that had come to my knowledge.

The result of these researches I now hasten to present to the Academy; and I do it with a double motive: to render homage to the author of the discovery, and also to give to philosophers who may follow this new path of science, certain indications, which they may possibly use with advantage, if, as I believe, they add something to what has been hitherto published.

The apparatus that I employed consists, 1st, of a Bunsen's battery; 2nd, of one or of several electro-magnets; 3rd, of one of M. Soleil's instruments, proper for manifesting the slightest angular displacements of the planes of polarization; 4th, of different substances to be subjected to experiment.

The elements of the Bunsen's battery are of ordinary dimensions: in most cases, it is sufficient to employ ten, in order to render the phenomenon sensible; but to render it measurable, to compare its intensities with a certain approximation, it is necessary to employ forty, fifty, and even a hundred elements.

The electro-magnets are those that I constructed fifteen year ago, and which can support as much as 800 k.g. (1764½ lbs.) when they are magnetized by a battery of 20 pairs. They are cylinders of soft iron, 7 or 8 c.m. (3 inches) in diameter, and about 50 c.m. in length, which are bent horse-shoe fashion, the distance of the axis of the two branches or poles being only 12 or 15 c.m. (4½ or 5 in.) There were 500 or 600 metres of copper wire, doubly covered with silk, rolled round each branch.

M. Soleil's instrument is described in the 20th volume of our *Comptes Rendus;* it is composed, as we know, of two parts, one objective, the other ocular.

The objective part, or that which is turned toward the light, is nothing more than a Nichol's prism, behind which is a system of two plates of quartz placed in juxtaposition, cemented at one edge and worked together, so as to fulfil the double condition of giving them exactly the same thickness, and rendering each of them very perpendicular to the axis. The surface of the junction of these

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plates being parallel to the pencil of light, and occupying the middle of its width, we see that the first half of the pencil traverses one of the plates alone, and the second half the other plate; and, as they have been selected with an opposite rotatory power, the first half of the polarized pencil is found to have its planes of polarization turned aside, for example, to the right by a certain angle; and the second half, on the contrary, is found to have its planes of polarization turned to the left in perfectly equal angular amplitudes. The extent of these deviations depends upon the common thickness of the two plates, which is habitually 5 or 6 mm.

The ocular part, or that which is turned toward the eye, first presents a fixed plate of rock crystal, also perpendicular to the axis, having, for example, a rotatory power to the right, and a thickness of 5 mm. very accurately determined by the spherometer. Behind this plate is placed the *compensator*, composed of two equal sized prismatic plates, endowed with the same rotatory power towards the left, viz., in a contrary direction to the former. These two prisms, opposed like two wedges, by their acute angle, are moved simultaneously by the same pinion; they slide upon each other so as to superpose, sometimes by their less, and sometimes by their greater thickness, and thus always form a system equivalent to a parallel plate, but to a parallel plate that would vary from the thickness of zero to a thickness almost double that of the base of each prism. To avoid the deviations that the light might undergo, on account of the variable distances of these prisms and the obliquity of their faces, each of them is compensated by a glass prism.

Finally, behind the compensator is a bi-refracting achromatic prism, and a small Galileo's telescope, against which the eye is applied in order to observe the pencil of light, which has traversed both the objective part, the intermediate bodies subjected to experiment, and the ocular part of the instrument.

The graduation of the compensator is easily made; and when it has been made with sufficient care, the instrument indicates that the cause, whatever it may be, which turns the plane of polarization has an intensity equivalent to that of a plate of quartz of a known thickness; always with the condition that this cause exercises, over different simple lights, actions comparable to that which the quartz itself exercises.

M. Soleil's instrument, the construction of which I have just described, must be separated into two parts for the

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experiments, of which I am about to speak. The objective and the ocular parts have been separately mounted on my diffractive bench (vide my *Eléments de Phys.* 4th edition, vol. ii. p. 126), which lends itself with the greatest facility to all researches in which it is required to centre the apparatus on the same axis.

An ordinary lamp is placed in front of the objective part, and a powerful lens gives a pencil of light sensibly parallel, which, in propagating itself along the common axis, traverses successively the object-glass, the pieces subjected to experiment, and the eye-piece; the distance between the object-glass and the eye-piece may vary within limits sufficiently wide apart; for it may extend to nearly 2 metres, or only to a few centimetres, according to the nature of the observations.

It is important to remark that the pencil of light is always horizontal, and that the apparatus has been accidentally arranged, so that the light is propagated from south to north, which may serve us to define more easily the relative positions of the polarized ray, of the electro-magnets, and of the bodies upon which they act.

The electro-magnet is horizontal, namely, the plane of the axis of its two branches is horizontal, and precisely at the height of the pencil of light, that traverses the apparatus; moreover, the vertical plane, formed by the extremities of the two branches, or poles of the electro-magnet, is parallel to this pencil, and may approach it more or less. This being fixed, if we wish to submit to experiment a parallelopipedon of flint glass 10 or 12 c.m. in length, and terminated perpendicularly to its length by two parallel planes, this parallelopipedon is first arranged in such a manner that the ray, polarized by the object-glass, traverses it according to its axis, and if it happens that the flint is pure and unannealed, as must be the case for the success of the experiment, its interposition produces neither deviation nor coloration in the ray of light.

The electro-magnet is then brought near by arranging it just as if the piece of flint were a piece of iron to serve as a *keeper*; and there is no harm in arranging it so that the two poles of the electro-magnet touch the flint; the middle of the length of the latter consequently corresponds to the interval existing between the two branches of the electro-magnet.

Things being in this condition, the current is passed; and suddenly we perceive that the two tints of the red image, that correspond to the two opposite plates of the quartz of

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the object-glass, cease to be identical; suppose, for example, that the one to the right had turned blue: if the current is made to pass in the contrary direction, it is the one to the left, which this time turns blue in the same manner. So also, on inverting the poles of the electro-magnet, the action exercised upon the flint, or upon the light that traverses it, is suddenly reversed.

Here then is the action in question rendered evident in the most striking and incontestible manner.

Under the circumstances of which I have just spoken, ten elements are more than sufficient to manifest it to an experienced eye; but with a hundred elements it requires such an intensity, that persons least accustomed to observations of this kind, will not fail of perceiving it as a perfectly characteristic phenomenon.

Before inquiring whether this effect, at once so new and extraordinary, results from an immediate action of the magnetic fluid upon light, or of a mediate action, in which the ponderable matter of flint-glass intervenes; or at least of the co-union of the forces, to which this matter is subjected, in order to constitute it in molecular equilibrium, we must first determine what is the nature of the effect produced, and above all things endeavour to measure its intensity, in order to know what the conditions are under which the phenomenon is manifested with the greatest energy.

For this purpose, instead of observing directly the coloured tints given by lamp-light by quartz perpendicular to its axis, we must re-compare what M. Biot has called *the transition tint* (*la teinte de passage*). This is obtained by placing in front of the object-glass various systems of blue and green glasses; but I have found, in the Museum of the Conservatory, some glasses slightly coloured blue, which gives this tint a still greater sensibility than is obtained by other means. When these glasses are interposed upon the pencil, the tints of the quartz became a clear lilac, upon which the least changes in the shades are appreciable; the uncertainties presented by the zero of the compensator then disappear, and it becomes possible, not only to perceive, but to measure the effects corresponding to thicknesses of quartz of the hundredth of a millimetre.

The instrument being thus modified, the compensator being at zero, and the polarizing prisms of the object-glass and the eye-piece being suitably regulated in their relative positions, we can proceed to experiment; only there is one point to which we must still pay attention, it is not to take any notice of the image that is yellow, and to regard exclu-

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sively the lilac image, the two halves of which are then exactly of the same shade.

Immediately that the current passes one of the halves of this image is seen, that to the right, for example, which turns blue; it will be seen that this tint is permanent, like the current itself; and we may convince ourselves that, from the very first, it has acquired all its value, namely, that the prolonged duration of the action makes no sensible addition to it.

The compensator is then to be moved in the proper direction; the difference of the tints is gradually effaced in proportion as it advances; and, with a little practice, it does not take any long time in finding the point where the equality is established. The number of divisions is then noted; and we thus obtain a measure, or at least an approximate measure, of the effect produced: say 20 divisions.

When the current is afterwards made to pass in the opposite direction, it is the other half of the tint, namely, that to the left, which turns blue, and the compensator must be moved in the opposite direction, in order to restore equality. No interval of time is appreciable between the change of the current and the change of the effect upon the light; and it is quite instantaneously that the shade acquires its full value. When the optical apparatus is well regulated, and the electrical communications are also good in each direction, the motion of the compensator is the same in both cases, namely, if it moved at first 20° to the right, it would afterwards move 20° to the left.

These opposite effects, and the corresponding measures, may be indefinitely repeated, either with the same or with a different number of battery pairs; and it requires but a few hours, while the battery action is tolerably constant, to pass in review a great number of diaphanous substances, and to have a fresh approximation to the relative sensibility with which they obey the magnetic influence.

When the substances submitted to experiment are more or less coloured, we must vary the systems of glass required for producing the transition tint; and we are not always successful in composing a tint equally delicate and easy of observation. It may consequently happen that substances even slightly coloured, when submitted to these means of observation, appear much less energetic than in reality they are.

Let us pause at diaphanous substances, and remark that, in the flint-glass experiment mentioned above, it was necessary to make the compensator move 20 divisions to the

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right, and 20 to the left, according as the current passed in one direction or the other. Let us remark that if, instead of interposing to the passage of the pencil a prism of flint, subjected to the electro-magnet, we had interposed, without magnetic action, a plate of quartz, perpendicular to the axis, and of a suitable thickness, turning to the right in the first case, and to the left in the second, it is certain that the equality of the tints would have been established by the same movement of the compensator. Now we know the effect produced by these plates of quartz would have been to turn the flame of polarization to the right and to the left; whence it seems very natural and very lawful to conclude, that the flint, when subjected to magnetic action, produced the same effect as these plates of quartz, namely, that it also caused the plane of polarization to turn to the right for one direction of the current, and to the left for the contrary direction. This, in fact, is the conclusion to which Mr. Faraday has arrived, and he has characterized this new action of magnetism upon light, by saying that magnetism turns the plane of the polarization of the luminous ray subjected to its influence under certain conditions, and that the direction of this rotation is connected with the direction of the current.

Quartz and other substances which of themselves, through their nature or structure, possess, without the concurrence of magnetism, the permanent property of turning the flame of polarization, exercise this action with variable intensities upon the divers elements constituting white light; and there are dispersive powers for this rotation, as there are different dispersive powers for refraction. It would be very important in this respect to go through researches upon the substances, which acquire this property by magnetic action, analogous to the very remarkable researches made by M. Biot upon the former. The apparatus, that I have employed, must be very much modified in order to be applicable to experiments of this kind; it is right to show the phenomena in a very sensible manner, rather than to measure them in their most delicate features. Furthermore, it is not upon phenomena so feebly developed as those which I have obtained, that a labour of this kind may be undertaken; for within these limits, they might be as readily explained by partial depolarizations toward the right and toward the left, as by the rotation itself of the plane of polarization; which, however, would detract nothing from their importance, and possibly might augment it.

As I have already mentioned in that specimen of flint

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which gave me the most energetic effects, the plane of polarization rotated by the magnetic action as much as it would have done by the action of a plate of quartz $\frac{2}{10}$ths of a millimetre in thickness; now, since by changing the direction of the current, the rotation occurs in opposite directions, we see that the total effect obtained in passing from the magnetic action, exercised in one direction, to that exercised in the other, is equal to that which would have been produced by a plate of quartz $\frac{4}{10}$ths of a millimetre in thickness.

This, at present, is the maximum effect, that I have been able to obtain. Since we have now a means of comparing the intensities of this force, it will be easy for us to see how it will be modified by the divers relative positions of the electro-magnet, and the piece of flint.

To this end, I have made the following observations:—

1st.—If, instead of placing the electro-magnet in contact with the piece of flint, it is moved parallel to itself in the same horizontal plane, and so that the vertical plane, by which the two corresponding branches are separated, is always in the middle of the flint, the action diminishes; but it diminishes feebly in proportion as the distance increases, so that even at a distance of 10 c.m. (4 in.) it is still a considerable portion of what it was at the contact itself.

2nd.—If, while the electro-magnet is again placed in contact, the piece of flint is made to slide in the direction of the ray of light, so as to submit it to the action of only one pole of the magnet, a moment arrives when the action is altogether null; then, if we continué to make it slide in the same direction, removing it more and more from its primitive position, so as to place it outside the pole, to which it is subjected, the action begins to rise again; but it is then contrary to what it was at first.

These observations appear to me to lead to three important consequences:—

It follows, first, that, if we consider the well known action of the magnet upon the flint as being produced by attraction and repulsion, the effect is null, when the resultant of these attractive and repelling forces is perpendicular to the direction of the polarized ray; and it is at its maximum when this resultant is parallel to the ray. From these considerations, we may also obtain a just idea of the direction in which it acts, for by considering, *still hypothetically*, the piece of flint as a piece of soft iron, acquiring two poles by the influence of the magnet, the movement of the plane of polarization is made to the right, when the light enters by the austral pole, and passes from the austral to the

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boreal pole; and, on the other hand, it is made to the left, when the light enters by the boreal pole. Consequently, whatever be the position of the piece of flint, if two observations are made upon it without touching it or in any way deranging the electrical apparatus, but by merely turning round the optical apparatus, so as to make the light enter successively in the two directions, we shall, in the first case, see the effect to the right, and in the second, the effect to the left, which proves, as Mr. Faraday pointed out, at least an apparent difference between substances which have the permanent property of rotating the planes of polarization, and those which acquire it by magnetic action.

It follows, in the second place, that, by operating in this way, we must guard against giving the pieces subjected to the electro-magnet a greater length than the distance of the axes of the two branches; for the portions that pass beyond these axes would receive modifications parallel to each other, and opposite to that which the central portion would receive, it could even be assumed that the compensation might be exactly made, so that, with a keeper surpassing the length of the magnet, the action might be altogether null.

This result appears to me opposed to that pointed out by Mr. Faraday, namely, that the effect is proportionate to the length of the piece subjected to experiment.

It follows, finally, that in order to obtain a greater effect, we might present to the piece of flint two electro-magnets opposed to each other, so that the poles of the same one were facing. This result I have also verified; and it is indeed by the aid of two electro-magnets thus opposed that I obtained the maximum effect spoken of above. By thus placing several systems parallel to each other upon the same pencil, the effect would undoubtedly be double, triple, &c.

It appeared to me of great importance to examine whether the position of the plane of polarization, in respect to the horizontal plane of the electro-magnet, had any influence over the energy of the action; but whether the plane of polarization be itself horizontal, vertical, or intermediate, the result appear to me to remain sensibly the same.

Hitherto I have only spoken of flint glass, but I have subjected to experiment all the other transparent bodies that I was able to procure; namely, flints of divers fabrics, and undoubtedly of divers compositions, crown glass and glasses of all kinds, coloured with copper, gold, chromium, &c., and then rock-salt.

All these bodies present, though with less intensity, the same phenomena as flint glass. Unfortunately, all the spe-

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cimens of crown glass were generally to a certain degree annealed, which modifies the colours, and does not permit of their being vigorously compared with other bodies; however, from the experiments that I have been enabled to make on certain less imperfect specimens, I am led to believe that the action of crown has an intensity comprised between the half and two-thirds of that of flint.

Chloride of sodium has an action very nearly allied to that of flint.

I also subjected to experiment certain transparent or coloured liquids; these experiments were made in a trough formed of parallel plates of glass, 13 c.m. (5 in.) in length, being equal to the distance of the axes of the electro-magnet, a width of 3 c.m. (1.1 in.), and a depth of 5 c.m. (2 in.). The trough being empty, and the electro-magnets being in action, there was no sensible effect produced by the parallel plates of glass that formed its extremities.

The intensity of all these liquids is about equal to that of crown; however, the most energetic seemed to me to be olive oil, distilled water, concentrated ammonia, pure nitric acid; and the least energetic, acetic acid, sulphuric acid, ferro-cyanide of potassium, ferro-cyanate of magnesia. It appeared to me certain that several bodies, when placed in solution with distilled water, weakened its effects.

Mr. Faraday announces that manganese, chromium, and cerum are magnetic after the manner of iron, and that all compounds of these bodies preserve in a greater or less degree this faculty. I had long ago established the former fact, in respect to manganese; and, in the course of last summer, I had proved it in respect to very pure chromium, obtained by the pile, either from chromic acid or sulphate of chromium. With regard to magnetic compounds, I had recently studied them by a very simple and easy process, which consists in placing upright a powerful electro-magnet, its poles upward forming a horizontal plane; a piece of thin paper is spread over each pole, in contact with the iron itself; and it is then sufficient to throw upon this paper a few very fine particles of the substance to be experimented upon, and to give the paper certain slight vibrations, which put them in motion. These particles arrange and fix themselves on the circle corresponding to the terminal edge of the iron of the electro-magnet and mark out this circle with great precision. By this means I have proved that almost all the compounds of the magnetic metals are more or less magnetic, Prussian blue and the sesquichloride of chromium (M. Peligot) are so in a remarkable manner. However,

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we meet with certain compounds that are rebellious to this means; such, for example, as double cyanide of iron and potassium, chromate of silver and bi-chromate of potash.

Other metals, as spongy platinum and arsenic, show a sensible action; but it would require being verified upon a perfectly purified specimen.

Bismuth presents other phenomena; instead of forming a circle, like the magnetic metals, it forms two concentric circles, thus leaving a narrow white band, in the very place where the other metals formed a circle, as if it were repelled by the more vivid action of the iron edge of the magnet. The effect is so marked that on mixing, for example, very finely powdered sesquichloride of chromium with bismuth also reduced to very fine powder, the violet circle of the chloride is seen, and also the two bismuth circles, which, although very near, are separated from it.

Amber appears to give, although much more feebly, the same appearances as bismuth.

No attractive or repulsive effect is observed by this means, either upon very pure antimony or upon the other metals and their binary or other compounds (among the raw metals, I have only tried tellurium and M. Peligot's uranium), nor upon the alkalis, sulphur, iodine, carbon, and diamond. I regretted not having at the moment at my disposal, either cerium or any of its compounds.

These negative results in no degree weaken Mr. Faraday's general proposition, as he undoubtedly operated by more delicate means, and with more energetic magnets. I give them here merely in order to point out the very easy process that I employed, and at the same time the limit of its sensibility.

There is another process for studying magnetic properties, namely, that formerly employed by Coulomb, when he discovered that all bodies are subject to the influence of the magnet, and which has since been employed for the same object by several philosophers, and very recently by Ed. Becquérel. (*Comptes Rendus*, vol. xx. p. 1708.) Mr. Faraday appears to have employed it; but, without doubt, on account of the weakness of my magnets, although excited by a pile of 100 pairs, I did not obtain the same results as he did: in my experiments, bismuth and amber are the only two substances which directed themselves perpendicularly to the line of the poles; and we shall undoubtedly be struck with the relation existing between this direction of bismuth and the effect of repulsion, which the fine powders of this body undergo on the part of the edge of the magnet.

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These two mechanical actions of magnetism upon bodies—the attraction and repulsion of fine powders, placed almost in contact with one of the poles—appear, therefore, to be dependent upon each other; but to what extent are they connected with the third action, the optical action, which Mr. Faraday has just discovered?

Admitting, with this philosopher, that all substances that are not magnetic after the manner of iron are *diamagnetic*, or magnetic after the manner of bismuth, we should be led immediately to the conclusion that the optical action being concomitant with a certain mechanical action, there is at least a presumption that this action is exercised upon the bodies, and not directly or immediately upon the light which traverses them.

But if it happens, as in my experiments, either on account of the relative weakness of my magnets, or the imperfection of the methods that I have employed, or through other causes—if it happens that glasses of divers natures, distilled water, fat bodies, &c., which are so sensible to optical action, are yet insensible to the mechanical action of magnetism, this would be no reason for concluding that magnetism acts directly upon light itself; a conclusion, moreover, which would have a precise meaning only in the system of emission; for in the system of undulations, which now seems so completely demonstrated, it is the ether of the body subjected to experiment which is modified by magnetism; and it would undoubtedly be a very difficult matter to recognise whether it is modified without any participation of the ponderable matter of the body with which it is so intimately connected.—[A.]

CXLI. M. Becquerel's *Observations in relation to the Action of Light upon all Bodies.* "Comptes Rendus," Jan. 26, 1846. Vol. xxii. p. 146, No. 4.

(Translation.)

In 1827 I communicated to the Academy a work, which I had just executed, upon the magnetic actions, excited in all bodies by the influence of very energetic magnets, taking up the question where Coulomb had left it. I began by showing that, if we submit to the action of a single bar a small paper cartridge, containing a mixture of sesquioxide of iron and magnetic oxide, in divers proportions, on placing the centre of suspension of the cartridge as near as possible in

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one extremity of the bar. This cartridge is immediately placed in a direction perpendicular to the line of the poles. On removing it from this direction it returns to it by a series of oscillations, the velocity of which depends upon the quantity of magnetic oxide that it contains. It follows from this, as may readily be seen, that the distribution of the magnetism is transverse instead of being longitudinal.

By bringing the centre of suspension within the bars, the cartridge deviates from the transverse direction, in order to approach the direction of the line of the poles. The transverse direction is maintained for a certain time, however small may be the quantity of magnetic oxide contained in the mixture.

In operating with sesquioxide of iron free from magnetic oxide, the cartridge also places itself in a transverse direction, when the point of suspension is very near to one end of the bars; by carrying this point within or beyond, the cartridge deviates from the direction, without, however, placing itself in the direction that is parallel to the line of the poles.

Wood, gum-lac, and any substance arranged in needles, give analogous effects; but as the magnetism of these substances is much more feeble than that of the sesquioxide, we must operate with two bars in order to obtain the effects. The needle of good gum-lac, or any other substance 4 c. m. (1½ in.) long, and about 2 or 3 (1 in.) in diameter, is placed between the two opposite poles of two strong magnets, the two ends of which are only at a few millimetres distance, the point of suspension being as near as possible to the interval, by which they are separated. The needle then places itself perpendicularly to the line of the poles, and not according to that direction, as in Coulomb's experiments; but, if we gradually withdraw the ends in front of the magnetic bars, it comes by degrees into the direction of the line of the poles. In the first case, a transverse distribution of the magnetism is obtained, as in the sesquioxide.

My son Edmond has recognised that, under certain circumstances in which the needles place themselves in the transverse direction, if we diminish their length without changing the distance from the poles of the magnets, they become less and less in the direction of the line of the poles.

From these facts it follows that the magnetic effects produced in steel or soft iron by the influence of a magnet, differ from those that occur in all other bodies; so that in the former the distribution of the magnetism occurs in the direction of the length, whilst in the tritoxide of iron, wood,

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gum-lac, &c., it occurs most commonly in the transverse direction, especially when they are cut into needles.

This difference of effect is connected with the magnetism's being very feeble in all these bodies; we may neglect the reaction of the particle of the bodies upon themselves.

NOTE.—M. DESPREZ *on Light and Magnetism.*

M. Desprez took the opportunity of rising to mention to the Academy that he had arranged several apparatus, by means of which he was desirous of seeking whether the action were manifested upon light.

The first of this consists in an iron gun several metres in length, enveloped in copper wire covered with silk. A vacuum may be made in this tube, and any gas introduced. Another apparatus consists in an analogous gun containing a glass tube in which the vacuum is made, and into which any body is introduced; finally, an electro-magnet, by means of which we may see whether, in the well-known experiment of the two openings and the bi-prism, the fringes are displaced by the electric action, as they are by the interposition of a plate, in M. Arago's experiment.—[A.]

CXLII. *Experiments on the Phenomena of Induced Contraction.* By M. MATTEUCCI.—Letter to M. Dumas. (Extracted into "Archives de l'Electricité," vol. v. p. 382, from "Annales de Chim. et de Phys." Sept. 1845.)

(TRANSLATION.)

You remember the experiment that I showed you on the contraction of the galvanoscopic frog, the nerve of which is laid upon the muscle of any animal in a state of contraction. This phenomenon has been termed in England, *induced contraction.* I have studied it of late to some extent; and it forms the subject of a long memoir which, in continuation of two others on the muscular current, and the proper current of the frog, has been honoured with insertion in the *Philosophical Transactions* of the Royal Society of London. I communicated the principal results of the first two memoirs to M. de Humboldt, and my letter appeared in the *Comptes Rendus.** I will now give you an extract from this third memor on induced contraction.

With a view of settling the point, whether this induced

* *Elect. Mag.*, Vol. II., p. 20.

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contraction might be due to a development of electricity in the contraction of the muscle, or more probably to a species of exterior discharge of the opposite electrical state, which produces the muscular current, I studied afresh the source of this current. I have just discovered that nitrous gas acts with still greater intensity than sulphuretted hydrogen in destroying the muscular current and the proper current. In like manner, I have found that, provided the muscular elements are left in contact with saline or alkaline solutions, the signs of the muscular current and of the proper current entirely disappear. Finally, I have lately searched out, both with the galvanometer and by using the galvanoscopic frog with great care, the existence of electrical states analogous to those of the muscle in the other organized tissues or materials of living or recently killed animals. I have obtained signs of the electric current by acting upon lungs, liver, and kidneys. The direction of the current is always from the interior of the organ to the surface in the organ itself. By comparing piles made with a similar number of elements either of muscles or of the organs above mentioned, a very remarkable difference is found in the intensity of the current. The muscular current is always the stronger and the more enduring. Acting on the cerebral substance or on the spinal marrow, I had much difficulty in obtaining, with the galvanoscopic frog, any signs of the current. I composed a pile of twenty elements, and according to my ordinary plan, with the spinal marrow of an ox recently killed, I obtained a current of 10° or 12° always directed, in the organized substance, from the interior to the surface. With a similar number of muscular elements, the intensity of the current would have been much stronger.

From all these facts, and from those that I had collected in my preceding labours, it appears to me impossible to entertain the least doubt as to the origin of the muscular current:—the chemical actions that occur in living organizations, and principally the action of arterial blood upon the organic elements. These chemical actions give rise to electrical conditions, which are developed and destroyed at the same time; and it is only by means of an experimental arrangement that we are able to create a kind of inequality in the circumstances of the production of the current and of the conductibility of the circuit, which manifests its presence. These circumstances are always verified in the interior and at the surface of muscular tissues, and the different parenchymes that I have named.

I cannot imagine how the celebrated physiologist of Berlin,

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Mr. Müller, could say, in his *Journal of Physiology*, that they had just discovered in Germany the general principles that can explain the facts, that I have discovered. These general principles are reduced to this: "An electric current is produced, when the transverse section of a muscle has just been put into communication with its longitudinal section by an arc." Rather than a general principle, it seems to me that there is nothing more in this than a vague and incomplete formula of my phenomena.

I think it useless to collect here all the observations that I might make in support of this truth, which, however, would be no very easy task to all who have followed my works. Moreover, I must add that, in reading M. Dubois-Raymond's memoir in *Poggendorff's Annalen*, I have not found that this physiologist, who is, as far as I know, the only one in Germany, who has confirmed and verified my experiments, has given any ideas different from my own on the origin of the muscular current and the proper current. It would have been desirable had this physiologist included in his work fewer hypothetical ideas; and also had he made more experiments. But, in general, I have found in different points of his memoir, that he speaks of the action of arterial blood upon muscular fibre. It is just to mention that he has the merit of having first pointed out, although in an incomplete manner, the part played by the tendinous substances in the proper current, as I have lately established it in my last memoir. I would also have wished that M. Dubois-Raymond had described in his memoirs the method that he had employed in studying the influence of gases upon the intensity and duration of the muscular current. The results to which I have arrived differ greatly from those of M. Dubois-Raymond; in fact, I have found that, in all gases, this current remains the same; and the variation produced by hydrogen is due to a secondary action, independent, consequently, of the nature of the pile. I have described, in the memoir that will appear in the *Philosophical Transactions*, the apparatus and the processes that I have employed in my experiments.

I regard as very exact the results to which I have arrived, but I have some difficulty in explaining to myself how, in working upon this subject, the difference produced by hydrogen gas, which is so manifest, has not been observed, whilst, when operating with other gases, differences have been found which do not really exist.

I now come to describe the principal results, to which I have arrived, when studying the phenomena of induced

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contraction, and investigating their cause. I have first varied the experiments in a thousand ways, in order to discover if there were any liberation of electricity in muscular contraction. For this purpose, I employed the most delicate instruments, and every possible care; and I must conclude that this liberation of electricity cannot be demonstrated by experiment. I have successively examined, trusting to experiment, the different hypotheses that may be framed to explain induced contraction, such as those of derived currents, induced currents, shocks, vibratory motions excited in the nerve by the muscle in a state of contraction, the different electrical states of the surface of the muscle and its interior, which may be supposed to be carried to the exterior by contraction, &c. All these hypotheses are overthrown by a great number of facts, among which I shall confine myself to reporting to you the following:—Among the bodies interposed between the surface of the muscle in contraction and the nerve of the galvanoscopic frog, which allow the induced contraction to pass, there are olive oil and Venice turpentine, rendered sufficiently liquid by mixture with the volatile oil of turpentine. The interposed film of these substances, which does not prevent induced contraction, is sufficient to arrest the passage of the muscular current, or of the current proper, and even that of an ordinary pile of tolerable strength. On the contrary, an excessively thin plate of mica, when interposed between the nerve and the muscle, prevents the induced contraction.

These facts are sufficient to prove that it is not by an electric action, either direct or indirect, developed by muscular contraction, that induced contraction can be explained. I have found that induced contraction remained, whatever were the direction of the nerve in respect to the muscular fibres; that the excitation of the induced nerve was propagated either toward the muscle or toward the nervous centre; that this excitation occurred, even when the induced nerve was already excited by any cause; that this induced contraction was capable of exciting a second and a third indirect contraction. We are hence led to conclude that induced contraction is a simple and primitive phenomenon, a new property of the nervous action, whose principal laws are established.

I cannot conceive how Mr. Müller could conclude from my works, that we are led to establish the identity of the nervous principle with electricity. I have, under a thousand circumstances, and in all my memoirs, insisted upon firmly establishing the analogies and the differences that exist be-

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tween these two principles; for this it is only necessary to read chapters VII. and VIII. of my *Traité des Phénoménes Electro-Physiologiques des Animaux.*

I shall add only one word more on the subject of induced contraction, in reference to the explanation of it given by M. Dubois-Raymond. This physiologist thinks that the muscular current is interrupted by the contraction; and that this interruption must determine *a rupture of equilibrium* in the nerve of the second preparation, and consequently bring about the contraction of the second muscle. It is with these same expressions that Mr. Müller reports the hypothesis made by M. Dubois-Raymond, in order to explain induced contraction. It was impossible for me to form any idea of the physical value of these expressions. It appears that M. Dubois-Raymond admits that the muscular or the proper current is weakened or interrupted during muscular contraction; but there is no fact in support of this idea. Galvani well knew that the signs of the proper contractions were weakened or disappeared in the frog suffering under tetanus. I have myself thoroughly confirmed this fact; but I must observe that this weakening is manifested, because the contraction of the frog is taken as an indication of the proper current, even when bending its leg or the lumbar nerves. But this is not found when measuring the proper current or the muscular current with the galvanometer. The difference is, therefore, due to the state of excitability of the nerve in the state of tetanus. It is only in this case, as I have noticed in my works, and afterwards verified, that the proof of the principle admitted by M. Dubois-Raymond may be found. I have found that frogs taken in the state of sur-excitation, developed by the employment of nux vomica, when prepared in the ordinary manner, and arranged as a pile, give a more feeble proper current than that which is obtained by acting upon frogs, that have not been subjected to the action of nux vomica. But if we reflect that the contractions do not remain in frogs prepared and arranged in a pile, we cannot see, in this single fact, the demonstration of the principle quoted by M. Dubois-Raymond. Moreover, it is difficult to conceive how chemical actions must become weakened in a muscle by the mere fact of its contraction.

If I have rightly apprehended the idea of the young physiologist of Berlin, put forth in his memoir already cited, it appears to me that we ought to admit, that the muscle which enters into contraction, by diminishing in engths and increasing in width, becomes, as far as the cur-

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rent is concerned, of less resistance; and hence arises the rupture of equilibrium which gives, according to him, induced contraction. But we are not aware of what current he is speaking; the muscular current and the proper current circulate in the muscular masses only in the case when these masses are cut and prepared. And, after all, I have just shown by experiments, that the induced contraction can never be produced by an electric action, either direct or induced. I repeat it, induced contraction is the first fact of an action at a distance, or more properly of induction exercised upon a nerve by a muscle in contraction. I think that this principle will possess some importance to the physics of the nervous system. If a nerve is excited in its contact with a muscle in contraction, it is natural that all the nerves which are ramified into a muscular mass must be exerted when this muscle enters into contraction by the stimulus of the nerve, that ramifies into it. It is easy to seize the applications of this latter consequence.—[A.]

CXLIII. *Experiments upon the Gymnotus Electricus.* By MM. D. De Miranda and G. M. Paci. Communicated to the "Scientific Congress of Naples," September, 1845. From "Archives de l'Electricité." Vol. v. p. 496, No. 20, January, 1846.

(Translation.)

The gymnotus, upon which the following experiments were made, arrived at Naples from Rio Janeiro, August 15, 1844. It is placed in a large metal trough filled with water; its food consists of frogs and small fish; when, for example, it wishes to eat a frog, it directs itself towards it; and, when it reaches to the distance of about a foot, it stops and looks at it. The latter becomes motionless, and, so to speak, paralysed, until it floats over the water without moving; the gymnotus then seizes it, and feeds upon it.

It is remarkable that this effect is produced upon a single frog among all those that are swimming round the gymnotus; and it is not always upon the one that is nearest to it.

The gymnotus has been sometimes seen to act, as Williamson describes it, namely to kill frogs or fish, as if with indifference, and without afterwards feeding upon them. This fact would seem to indicate that the only object which the animal has in view, in certain cases, is to discharge its electricity when it feels a need of doing so.

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At the period of cold weather, the gymnotus was transferred to a wooden box, placed in one of the chambers of the royal apartments. This apartment, although exposed to the south, was very rarely opened during the winter; and then only for the few moments necessary for changing the air.

The water in which the animal naturally lives is muddy. In endeavouring to reproduce this circumstance, the water of the trough was entirely changed only once a fortnight; in the intervals it was partially filled with common warm water. Thanks to these precautions, the gymnotus, which was originally from a warm climate, and accustomed to live in water at 26° cent. (80° Fahr.) passed a winter easily in a bath at the temperature of 22° to 24°, and in an atmosphere of 24° to 25°. It attained the length of 3 ft. 4 in. The largest gymnoti that Humboldt observed at Cano de Bera, in South America, were 5 ft. 3 in.

Our attention is first directed to the phenomena of the shock. The shock is felt most powerfully when the animal is touched with both hands on two different parts of its body. In this case it is felt in the arms and in the breast, which form an arc of communication; but it is also felt, even when the animal is touched with a single hand; it is then felt in the arm that is in contact with the animal, and in the correspondent articulations; the discharge traverses the limbs, the ground, and the water of the bath. If a chain of several persons is formed they all feel the shock at the same time, whatever be the manner by which the discharge is obtained, namely, whether the first and the last person of the chain touch the animal at the same time, or one person alone places himself in contact with it. In the latter case, we have seen the shock communicated to four persons. Two persons having then touched the gymnotus, one towards the head, and the other towards the tail, one only felt the shock,—the one who touched the tail; at another time it was the one who touched the head: again at other times both felt it, and more powerful (as they fancied) than when either of them had alone felt it.

The gymnotus does not make the shock to be felt every time it is touched. In order to obtain it, it is sometimes necessary to irritate the animal, even to squeeze it, especially near the electric organ,—a fact which proves, as Bancroft says, that its electro-motive apparatus is not put in action except by the concurrence of its own will. Sometimes, when it has been irritated, the shocks that it gives are so energetic, that they surpass those of the strongest

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piles, and they who receive them feel their effects for a considerable time; they really become painful when they are repeated. We are aware that they are more or less energetic according to the degree of vigour and of the excitation of the animal; they are also more intense before its meal than when it had eaten. Their intensity diminishes in proportion as the animal is exhausted; so that, when it has been excited for some time, it is necessary to leave it at rest, in order that it may recover its strength.

It might be said that the gymnotus is endowed with an occult sense, which informs it of the degree of vigour of the person or animal that is to receive the shock, so that it may regulate the intensity of its discharge. This fact agrees with De Humboldt's observations, who says that the gymnoti know how to choose in large animals for the particular place that is most sensitive to the shock.

A workman of the king's household, who was suffering with a very violent pain of the right shoulder and the right arm, and being almost incapable of attending to any duty, was entirely restored after receiving three or four shocks of mean strength. The same was the case with a servant of the royal apartment, who was suffering with a rheumatic affection.

When the gymnotus was transferred to a small tub, the water of which was gradually raised to 28° C. (82° Fah.), the shocks that it gave were stronger and more frequent in proportion as the temperature of the water increased,—a fact which confirms the account of travellers on the greater intensity of the phenomena produced by this fish in its native country, and in a warmer climate. We cannot, however, say, with Becquérel, that there is any analogy between the calorific action of the electric discharge, and the phenomena of phosphorescence; we should rather say, with Professor Matteucci, of Pisa, that the greater activity of the respiration and circulation of the animal, while living in warm water, has a direct influence over the nervous system, and hence over the electric functions.

The prepared frog contracts strongly, when the gymnotus discharges itself even in a great mass of water, and at a considerable distance. It is by a similar effect that the animal renders itself master of the fishes, upon which it feeds. We have more than once observed the following fact:—when a fish is held in the pond, and made to move slowly through the water, in order that the gymnotus may follow it, the latter, when it becomes tired of this amusement, would discharge its shock and seize the fish, of which

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we were obliged to loose our hold, on account of the violence of the shock.

We have frequently received shocks, when touching the animal with a small metal rod. I have, however, met with some individuals, who did not feel it when they held the rod by one extremity, and even irritated the animal with the other. We made the same observations by insulating the fish. These facts confirm the opinion that the latter is warned by an intimate sense of the possibility or impossibility of the electric circulation; when it feels that it is impossible, it does not liberate its discharge, however provoked it may otherwise be; or, if he make the attempt, it is reduced to a slight phenomenon of tension. These facts would equally accord with the observation that the discharge is proportionate to the degree of sensibility of the individual who receives it.

Muschenbrœk was the first who furnished a sound argument to prove the electrical nature of these shocks, by showing that there was no discharge when the fish, that could produce them, were touched with non-conducting bodies. Walsh, Ingenhouss, and Fahlberg were the first to obtain the electric spark from the gymnotus, by interrupting the conducting circuit with two small gold leaves placed upon glass, at the distance of a line from each other. But we obtained it in a much more brilliant and decisive manner. The apparatus that we employed was a small bent glass tube, the two branches of which were equal, and which was filled with mercury in the bent part; we fixed at each extremity a long copper rheophore covered with silk, one of the ends of which descended into each tube as far as within a line of mercury. The opposite extremities of these wires were attached to two of those wooden handles, furnished with sponges, that are particularly employed in the medical experiments, made with Clarke's large magneto-electrical apparatus. On exciting the gymnotus with the sponge upon the back and upon the abdomen, in points corresponding to the electrical organ, and operating in the dark, we perceive the electric light burning on the surface of the mercury, which we gently agitated. When we introduced in this circuit Henry's multiplying helix, the sparks were greater and more brilliant.

When we had interposed in the circuit a helix which had an unmagnetized steel needle in its interior, this needle was magnetized after a few discharges. This fact agrees with what Faraday observed in London, upon the same fish, in 1839; and which Profs. Configliacchi, Barlocci, and Linari

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had previously observed in Italy with the ichthyochemical current of the torpedo. From the experiment made by Schœnbein and Faraday to decompose the iodide of potassium by the electricity of the gymnotus, we endeavoured to place in communication with the above-mentioned rheophores Clarke's large apparatus for the decomposition of salts. We not only obtained the decomposition of the iodide of potassium, but that of nitrate of silver and acetate of lead. The two free extremities of the rheophores were terminated by platinum points; the latter were plunged into a porcelain capsule, containing the solution of nitrate of silver; the decomposition of the salt was not long in being brought about.

We in like manner produced with the gymnotus Nobili's uniform tints and Newton's coloured rings, which Linari had obtained by the electric discharge of the torpedo.

Davy and Linari were the first to discover the calorific power of *ichthyo-electric currents*, the former by employing Harris's electrometer, the latter by employing Breguet's thermometer. In the experiments made on the London gymnotus in 1839, Schœnbein attempted to discover the increase of temperature with Snow Harris's thermo-electric apparatus; but this apparatus having got out of order, he could not continue the experiment. However, Faraday thought that he perceived some indication of heat in the currents of the gymnotus; this induced us to renew the experiment with Melloni's thermo-multiplier. We united a very thin silver wire by its two extremities to two conducting wires; the heat produced by the passage of the ichthyo-electric current was not sufficient to make the wire red-hot; but it heated sufficiently for the thermo-electric deviation of the galvanometer to be promptly manifested.

Walsh, and other philosophers after him, were disappointed in their attempts to observe whether the electricity of fishes presented any phenomena of tension. Linari, however, in 1836, obtained, in the gold leaves of the condensing electrometer, evident signs of positive and negative tension, according as the conductor communicated with the back or belly of the torpedo at the moment of contraction. With regard to ourselves, we were not able to obtain with the gymnotus any phenomenon whatever of electric tension, neither with Singer's electroscope, nor with Bohnenberger's, nor yet with Perego's more recent instrument.*

The same is not the case with galvanometric deviations; we verified on the Naples gymnotus, the facts which had been

* Vide Mr. Gassiot's Experiments in "Transactions of Electrical Society."

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observed on the London one, by Schœnbein and Faraday, and on the torpedo by Blainville, Flourens, Barlocci, Linari, Matteucci, Zantedeschi, and Prudente. We frequently obtained deviations of more than 90°, which could not be measured by the multiplying apparatus. The needle deviated briskly, as if it had received the impulse of a mechanical blow. The action was so intense as to alter the needle of the galvanometer so as to render it unfit for other experiments. The same fact occurred to Linari, to Zantedeschi, and to Prudente, with the current of the torpedo. Better informed than they, we employed with advantage Majocchi's Universal Galvanometer;* we, in like manner, had occasion to verify the phenomenon observed by Zantedeschi, namely, that it is not necessary to put the conductors into contact with the animal, but that it is enough to plunge them into the water, even at the distance of several inches from the gymnotus, to obtain a considerable deviation at the moment of the discharge. The same result is also obtained with a closed circuit, by placing the hands upon the back and the belly of the gymnotus. Is this an effect of the great diffusion of electricity observed by Schilling and then by Walsh upon the gymnotus, and afterwards by Matteucci and Zantedeschi upon the torpedo?

Since 1836, Linari obtained the most distinct phenomena of induction by the assistance of the electric discharge of the torpedo. The apparatus that he employed was nothing more than a system of two helices and three concentric galvanometer coils. The interior helix was electro-magnetic; the exterior was simply a spiral. The extremities of all the wires, of which the helices and the galvanometers were formed, were metallically united on the two sides of the helices and the coils. They thus formed the extremities of a single wire in a helix, the thickness of which was equal to the sum of all the wires. Linari obtained galvanometric deviations, magnetization, and the induced spark, by means of this apparatus, which, as we see, was nothing else than a magneto-electro-dynamic spiral, or a large solenoid. A similar arrangement with the gymnotus gave us the same results, namely, an indication of the galvanometer, and feeble magnetization of the needles. Had we excited the animal to a greater extent, we might, in like manner, have obtained the spark. When we placed the back and the abdomen of the gymnotus in communication with Callan's electro-magnetic machine, we immediately obtained a very sensible shock by the induced current.

* Vide *Elect. Mag.* Vol. I. p. 275.

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We verified, by galvanometric deviations and by chemical actions, a law discovered by Breschet and Becquérel, and confirmed by Matteucci, but which is opposed to the opinions of Davy and Walsh, namely, that all the points of the dorsal part of the organ are positive in respect to those of the abdominal part.* We also verified the fact pointed out in London by Faraday and Schœnbein, that the head or anterior part is positive in respect to the tail or posterior part. Finally, we established on the gymnotus Prudente's observation on the torpedo, that the electric currents are transmitted in the direction of the muscular fibres. This observation confirms that of Prevost, that the electro-physiological current in the muscles is from above downward, namely, from the head to the extremities.

Such are the facts that we have hitherto been enabled to verify upon the extraordinary animal that has been transported for the first time into our climate. We hope that we shall not rest here in our researches. Will it be granted to us to observe any new facts, and to contribute to the discovery of any new law, which may reveal the causes of ichthyo-electricity?

The Editor's Observations. By M. De la Rive.

MM. De Miranda and Paci particularly insist in their memoir upon a fact, which we cannot interpret as they do, namely, that the gymnotus directs its current where it wills, and in proportion to the intensity of the resistance that it experiences. We believe that this is an illusion, and that the same consequence may be deduced from the phenomena produced by an ordinary pile, which we cannot suppose to be endowed with will.

The gymnotus gives its discharge when it wishes; this fact is indisputable. The resulting current goes from the head to the tail in the body of the animal, and from the tail to the head in the water; the tail and head are like the two poles of a pile. This current, in traversing the liquid, becomes diffused in it, and traverses all the conducting bodies that it meets. Thus every animal, placed in the water, in the route of the current, will be traversed by it. The gymnotus may, by means of the flexibility of its body, even bend itself so that its head and tail, and consequently the current, shall be in the right line that passes through an

* With the assistance of Prof. Prudente for the physiological part.

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animal; in this sense only may it be said that the gymnotus directs its current.

With regard to the supposition that the gymnotus apportions the intensity of its charge to the resistance that it experiences, we believe that the facts upon which this assertion is founded are explained naturally enough when we call to mind that the effect of the current is always more sensible in the points where the resistance is greatest. And with regard to the choice that the gymnotus makes of the part, which in each animal would be most sensitive to the shock, in order to direct its current there, this is also an illusion, which is due to the fact, that the same current that passes everywhere, if, at least, conductibility permits it, produces at that place a more marked effect.

I may add, that having had an opportunity of seeing at Naples the gymnotus in question in MM. Paci and De Miranda's memoir, I cannot give a better account of the sensation that I experienced from the shock it gave, than by comparing it with that produced by an indirect current, or a battery composed of numerous elements, and charged with badly conducting liquids. It has no relation to the effect of the discharge of a Leyden jar.

One circumstance, which still more assimilates the current of the gymnotus to that produced by a pile, is, that if the animal is touched, on the one hand, in the middle of the body, and, on the other, at the head or tail, we obtain in the galvanometer a current which has just one-half the intensity of that developed when the two points touched are the head and tail.—Aug. De la Rive.—[A.]

CXLIV. *Atmospheric Electricity.* By M. Arago. From a Memoir "On the Impossibility of Predicting the Weather," in the "Annuaire" for 1846.

(Translation.)

It would be difficult not to include electricity among the causes which have a notable influence over climatological phenomena. Let us go further and see if the works of man may interfere with the electrical state of an entire country.

The clearage of a mountain is the destruction of a number of lightning conductors, equal to the number of trees that have been felled—is the modification of the electric state of an entire country,—is the accumulation of one of the elements indispensable to the formation of hail, in a locality, where, at first, this element was inevitably dissipated by the silent and

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incessant action of the trees. Observations come in support of these theoretic deductions.

From statistical details, the losses occasioned by hail in the continental states of the king of Sardinia, from 1820 to 1828 inclusive, amount to the sum of forty-six millions of francs. Three provinces, that of the *Val d'Aoste*, of the *Vallée de Suze*, and *La Haute Maurienne*, do not figure in these tables: they did not suffer by hail. *These three provinces have their mountains more covered with wood.*

In the hotter provinces, that of Gênes, whose mountains are well peopled, is scarcely ever visited by the meteor.

Atmospheric electricity gives rise to phenomena immense as to their extent; however, they seem to have originated in causes purely local. Their propagation operates also under influences circumscribed by zones that are special and sometimes very limited.

On the morning of July 13, 1788, a hail-storm commenced *in the South of France;* in a few hours it traversed the entire length of the kingdom; and then extended into the lower countries, and into Holland.

All the regions in France that suffer by hail, were situated in *two* parallel *bands* directed from the S. W. to the N. E. One of these bands was 175 leagues long; the other, about 200.

The mean length of the more western hail-band was four leagues; the other, only two leagues. The interval comprised between these two bands, received only rain: its mean width was five leagues. The storm moved from S. to N. at a velocity of sixteen leagues per hour.

The destruction occasioned in France in the 1309 parishes that suffered by hail, amounted, according to an official inquiry, to twenty-five millions (francs.)

Here, assuredly, is a plague, an atmospheric disturbance, considerable either for the material damage it produces, or for the influence, which the displacement of the air, and the mass of air deposited upon the surface of the two long and wide bands of country, must exercise over the normal temperatures of a great number of places. Even supposing meteorologists to be as well instructed as possible, would it be in their power to predict this?

The two bands *had their origin* in the *country of Aunis*, and in *La Saintange*. Why there and not elsewhere? Why did not the storm commence upon any other parallel of latitude, passing through its southern extremities? They reply, it is

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because in Aunis, and in Saintange, on July 13, 1788, the conditions of *electricity and temperature* were eminently favourable to the production of a hail-storm and of a concomitant hurricane, directed from the S.S.W. to the N.N.E. Agreed:—but were not these thermic and electric conditions, so favourable to the birth of a hail-storm, intimately connected with agricultural operations, with the existence of such or such a mass of trees, with the state of the irrigations, with circumstances variable according to the wants or caprices of men? With regard to the temperature, the reply would not be doubtful to any one. On the other point, the link will not seem the less evident, if I recall to mind that evaporation is an abundant source of electricity, and that various philosophers have ever ranked vegetation among the generating causes of this same fluid in the atmosphere.

If it is true, as it is thought to have been recognised, that, *in certain cases*, the flame and smoke that escape from the mouth of a high furnace or from the chimney of a manufactory can deprive the atmosphere of all its electricity for a great number of leagues round, the *prophets* in meteorology would find themselves in fresh embarrassment; they must know before hand the projects of the owners of forges, and the proprietors of manufactories.

From what is certainly known of the physical cause of *trombes;* according to M. Espy's theory, the ascending current, generated by the chimney of a manufactory, would sometimes be sufficient to give rise to one of these formidable meteors.—[A.]

CXLV. *Note on the Conductibility of the Earth for the Electric Current.* By M. Ch. Matteucci. Extract from a Letter to M. Arago.—From "Comptes Rendus," Jan. 12, 1846. Vol. xxii. p. 86. No. 2.

(Translation.)

After having *proved* by a great number of experiments the good conductibility of the earth for an electric current, it remained to *explain* this property in a body, the conducting power of which is so feeble.

Two hypotheses were put forth before the scientific congress at Milan, when the results of the experiments that I had attempted upon this subject on the road from Milan to Monza were presented. It appears that in a discussion that

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occurred on the same subject at the Academy of Sciences, these same two hypotheses were revived and advocated. Must we regard the earth as every other conducting body, which, by its great volume, may make up for its bad conductibility? Or rather, must we admit that the two electric charges, liberated at the extremities of the pile, always find means of diffusing themselves into the earth, which, being a universal reservoir, succeeds in neutralizing these charges with its natural fluid, decomposed by the free fluids of the pile?

This note contains the description of certain experiments, which appear to me of a nature to prove in favour of the former hypothesis. Let us imagine a long interval of ground, traversed by the electric current; and let us seek for derived currents in this interval. The following is the experiment:—I established a circuit of a pile of 10 Bunsen's elements, by plunging the two poles in two wells 160 metres apart. A galvanometer was in the circuit to ensure the passage of the current. In this interval were two other wells, and almost in a straight line with the two extreme wells. The distance between these two wells was 30 metres; they were distant from the two extreme wells, one 80 metres, the other 50. I plunged the extremities of a good long-wire galvanometer into the two intermediate wells: these extremities were either of silver or of platinum; I always waited until the needle of the galvanometer returned to zero. I then passed the current in the great circuit; and I instantly obtained a deviation of 35° or 40°. I repeated the experiments after having inverted the direction of the current of the battery in the great circuit; immediately the direction of the current, which I shall henceforth term *derived*, is also inverted. I beforehand assured myself of the perfect insulation of my two circuits.

Finally, I repeated the experiments, by reducing the interval of derivation to the length of a metre, namely, by plunging the extremities of the galvanometer in the same well. In this case, on closing the battery circuit, I obtained a deviation of only 3° or 4°; but this was also reversed, on changing the direction of the battery current.

It is, therefore, very well proved that the currents obtained in the intermediate circuit were derived currents. Now, this ought to be the case, if we admit that the electric current is transmitted to them each in the ordinary manner, whilst it cannot be conceived under the other hypothesis.

It is clear that the neutralization of the two free fluids at the extremities of the pile would not simply disturb the

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natural fluids of the mass of each interposed between these extremities; but that this would occur in every direction around these extremities.

If we admit the neutralization of the two electricities in the interposed mass of earth, operating successively from molecule to molecule, we then admit the hypothesis that we always make for the propagation of electric currents.

I continue occupying myself upon the possibility of establishing an electric telegraph across the sea; it appears that the loss of the current, transmitted by copper wire, plunged into water, increases rapidly with distance. I will take the liberty of giving you an account of it, when I shall have arrived at more conclusive results.—[A.]

CXLVI. *Construction of a very Powerful Magnet by Induction, without the Employment of Electric Currents.* By M. BABINET. From "Comptes Rendus," Feb. 2, 1846. Vol. xx. No. 5.

(TRANSLATION.)

I TAKE a bar of very soft iron, 4 or 5 decim. (15 to 20 in.) long, by 5 or 6 m.m. ($\frac{2}{10}$ in.) in thickness, and 15 or 20 m.m. ($\frac{1}{2}$ to $\frac{3}{4}$ in.) wide. I suppose it placed horizontally upon a table, so that its extremity passes about 2 or 3 c.m. (1 in.) beyond the edge of the table.

I place in contact with the non-salient extremity a magnetic bar, situated in the prolongation of this bar, *of the same transverse dimensions as it has*, but the length of which is only 15 or 20 centimetres (6 to 8 in.). Then, by induction, the soft iron bar acquires at its free extremity a pole of the same nature as the pole of the magnetic bar placed in contact with the other extremity. I then placed transversely on each side of the soft iron bar, twenty-four other similar magnetic bars (twelve on each side) which I brought into contact with the part of the bar near to the magnet that had already been placed in contact with it, so that toward this part opposed to that which reaches beyond the edge of the table, twenty-five poles of magnetic bars, all of the same nature, are acting concurrently to produce a very energetic pole at the free extremity. This, in fact, is the result that is obtained.

The following are the various effects observed:—

1st.—The pole of this induced magnet sustains enormous weights, when they are suspended to a soft iron keeper.

2nd.—If a soft iron bar is made to adhere to this pole, the attraction is such that, in drawing it along the end of the pole that hangs over the table, the bar that is forced to slide

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gripes, and is filed by the polar extremity of the horizontal bar.

3rd.—A soft iron bar, 50 c.m. (20 in.) in length, is seized by the pole at its lower extremity, and is held *straight above* this pole (and not pendant, as is commonly the case). The latter also sustains, above its upper extremity, by lateral adherence, a second similar vertical bar, of 25 or 30 c.m. (9 to 12 in.) in height; and this latter sustains, also vertically, by lateral adhesion, a very large key, that surmounts the two preceding bars.

4th.—Steel bars are powerfully magnetised in passing them over the free pole of this system.

5th.—I made the bar of ordinary Swedish iron, which is esteemed excellent for magnetic use; but, in the hope of obtaining still softer iron, I also endeavoured to employ iron wire forged together, which gave me an iron of a superior *tone;* but I obtained no good with this new bar, perhaps for want of its being softened. It is also well known that, for electro-magnets, care must be taken not to forge the prismatic bars of commerce; and that to give them a circular section, as is the custom, the salient angles must be taken away with the file, and not with the hammer.

6th.—We might place upon the soft iron bar a greater number of magnets; for there is nothing to prevent our placing them perpendicularly to its horizontal surface, both above and below. The soft iron bar intended for induction might be placed vertically, and present six or eight surfaces, which would each receive the contact of several magnets; finally, to the pole, that we desire to produce, we might adapt several branches of soft iron, which could each be subjected to the influence of several magnets. Perhaps we might thus arrive at the limit of the magnetic power, which would be admitted by a given extremity of a system, magnetized by induction. I confine myself here to pointing out that the action of about two hundred small magnets, of the size of those that are obtained from fragments of the blades of fencing-foils, did not produce a result nearly so satisfactory as the twenty-five magnetic bars alluded to above. From several known facts, I think that if the pole of the inducing bars were planted in grooves made in the soft iron rod, the effect of induction would be still more intense.

7th.—If the poles of the magnets are brought no nearer to the horizontal bar than 2 or 3 c.m. ($\frac{3}{4}$ to 1 in.), the induction is still very energetic; and by approaching or withdrawing, by a sliding motion, the system of magnetized bars, a temporary pole is obtained in the bar subjected to induc-

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tion, of variable power, which reproduced the phenomena of Faraday's temporary actions.

8th.—It is also on account of Mr. Faraday's recent experiment, on the magnetic effect produced in transparent bodies not magnetic by the ordinary means, that I recall to mind the experiment that I published fifteen or twenty years ago, and showed to the Philomatic Society. We could place at the two extremities of the substances traversed by the polarized ray, the extremities of two soft iron bars, pierced by a pole of sufficient diameter, and would then render these bars magnetic by a sufficient number of magnets placed transversely; the alteration of the poles, moreover, could be made without difficulty, and without having need to displace the bars, by merely altering the poles of the inducing bars.—[A.]

CXLVII. *On Electro-Culture.* By Mr. Pearsall; once Assistant to Dr. Faraday. Communicated to the Hull Literary and Philosophical Society, Dec. 9, 1846. From "Eastern Counties Herald" of Dec. 11.

An outline was given of this experimental mode of using wires to convey electricity to an enclosed area of ground; the accounts of wondrous effects said to be produceable by these simple means went the round of the agricultural press at the commencement of the year. Mr. Pearsall had personally opportunity afforded of laying down the plans as proposed by R. D.Forster, of Findrassie, near Elgin—and he had endeavoured to follow the plan faithfully—but no effect on the electrified portion at all could be discovered during the summer and autumn. He had learned of several arrangements having been tried with similar sincerity, and with uniform want of result, viz., by Mr. Mansfield Harrison, Keyingham Marsh; Messrs. William and Joseph Stickney, near Ridgmont; W. Torr, Riby; &c. &c. He therefore wished to ascertain what benefit had resulted from the observations made by different observers, supposed to be from 1,000 to 2,000, who had tried it, and learn also whether the failures in the East Riding and North Lincolnshire were owing to any want of *minute* attentions, or to any ill adaptation of soil, or peculiarity of climate, or lastly, whether the failure was due to the demerits of the plan itself. He therefore had endeavoured to learn the results obtained from others whose efforts were conducted in a first-rate manner, and with different circumstances of soil and climate. Mr. Pearsall then read the

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valuable opinions he had been favoured with from R. Hunt, Esq., F.R.S.; Edward Solly, Esq., experimental chemist to the Horticultural Society, London; J. Mechi, Esq.,* of Tiptree Hall, Essex (whose arrangement had been personally inspected and approved of by Dr. Forster), Chas. Charnock, Esq., Ferrybridge; Wm. Torr, of Riby; and other authorities. The uniform tenor was disappointment and failure. Mr. Torr was the only one who offered his results with accuracy and figures; he had carefully measured the ground, and similar areas, electrified and non-electrified, and after the utmost diligence he had obtained *one peck per acre* of barley in favour of the electro-culture, instead of the increase announced of thirteen quarters of barley having been had, while non-electrified portions yielded five quarters, and the statement made that "any mortal having ground, seed, and firmness of purpose, had the power of equalling this result."† Mr. Pearsall slightly adverted to the influence of electricity on vegetation, and while he was impressed strongly with the idea that it was the power with most economy and ease of measuring and regulation to be applied on the large scale, yet that the observations and decisions as yet left the case precisely in the same state. He then adverted to the influence of the decomposing power of the electricity over the substances, salts, and fluids of the soil, and plants—adverted to the presence or absence of light—the heat of the current itself, and the different stages of growth, as all being required to be named when advancing any views from a single or small experiment. He was not aware that any public account had been given of the results of electro-culture before, but he took this public mode of endeavouring to obtain the benefits of experience, from this handsome trial of an experiment, which had been propounded in language that afforded the practical or theoretical man no opportunity to benefit by any previous knowledge. He thought, in bringing forward these results and adducing these high authorities, he had done something to relieve the unpleasantness arising from individual failures, and thought

* I laid down experimental wires on various portions of my Tiptree Hall farm, the plots varying in size from one to seven acres, the whole extent being forty-four acres. So far as these experiments go, there is an entire failure, the crops non-electrified being quite equal to the enclosed plots. In April, whilst my man was connecting the wires, he got three smart electrical shocks, in each case from the wire over the pole, proving the occasional passage of a considerable current of electricity. Still no beneficial results are shown. The laying down of the wires was seen and approved by Dr. Forster himself.—*J. J. Mechi*, 4, *Leadenhall-street*.

† *Agricultural Gazette*, May 31.

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that these extensive and prompt trials showed that the agricultural world were active, and willingly disposed to plans that were proposed with authority for general good.

The President (Dr. Horner) said he was taken quite by surprise by the weight of authority adduced; he had no idea that the failure had been so complete; he had expected better things. At that time he had experiments in progress with galvanism, and he most certainly should make known the results to the Society.

The Rev. Ed. Higginson suggested whether the situation of the original experiments (North Britain) and the possible level, elevation, and climate, and even the variable climate for this year, might not be some means of explaining the discrepancy.

To which Mr. Pearsall replied, that if the plan had been proposed with these conditions, then questions might be entertained; but the plan was given in the decided language of experience, permitting no alteration in the particular mode, referring to cases where it would be tried, and giving facts and figures, and stating any "mortal, with conveniences, could produce like results," and it appeared that all this valuable time, and expense, and observation, amounted to something like pursuing a mistake or hoax.

CXLVIII. *On Magnetic Vibrations.* By William Beatson, Esq.

(*To the Editor of the Electrical Magazine.*)

Sir,—In an account* of a few experiments showing the vibrations excited in conducting wires, when traversed by electric currents, it was suggested as probable, that these vibrations were caused, or at least accompanied, by a certain molecular disturbance, producing expansion of the conducting wire, distinct from the expansion effected by the calorific action of the voltaic current. I had not at that time seen any notice of the experiments of M. Wertheim,† who has stated that such an expansive action is produced; but, as that philosopher does not mention, in the accounts which I have seen, that the degree of this electrical expansion is different, according as various metallic conductors are employed, I may perhaps be excused, in describing the apparatus by which I have observed that this is the result, in accordance with a supposition naturally suggested, by

* Vide *Elect. Mag.* Vol. I. p. 555. † Ibid. p. 605.

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noticing the much greater vibrations produced in some metals than in others.

In order to observe this expansion with definite accuracy, a very light but stiff beam of wood is arranged in a horizontal position and fixed near one end, in the manner of a steel-yard, and the conducting wire, which is in a perpendicular direction, is attached near to the fixed end of the beam, so as to cause the other end to move over a considerable space for every slight increase or dimnution in the length of the conducting wire. In this manner, an expansion of $\frac{1}{10}.\frac{1}{000}$ of an inch is rendered evident; and, when necessary, the effect produced is still more delicately shown, by using a microscope and proportionate scale. Thus, the only tension to which the wire is exposed, is that caused by the slight beam to which it is attached.

When conductors of different metals are experimented upon, the two wires which are to be compared are both included in the circuit, so that each can be alternately placed in connection with the beam, and thus exact comparative results be obtained. The length of the wire to be tried is 2 feet; and the beam is so proportioned, that an expansion of $\frac{1}{2250}$ of an inch in the conductor, causes the index to move over a space of $\frac{1}{10}$ of an inch, being equivalent to an expansion of $\frac{1}{60}.\frac{1}{000}$ of its whole length. The wire under experiment is all enclosed in a helix, consisting of a small glass tube of the length of the wire, and having a coil of covered wire round it from end to end. The connections are so arranged as to allow of the same voltaic current being passed through both wire and helix; or, as was generally preferred, separate voltaic currents through each.

When a copper wire was subjected to the influence of the coil, no perceptible effect was produced; when the current was passed through the wire itself, a slight sudden expansion took place, which was immediately merged in the regular expansion produced by the calorific action of the current. The total expansion which the heating power of the current effected, amounted to $\frac{1}{300}$ of an inch: the sudden expansion was scarcely appreciable.

An *unannealed* iron wire, when subjected to the influence of the helix, exhibited a sudden expansion of about $\frac{1}{2000}$ of an inch; and, on suspending the influence of the coil, a contraction of the wire to the same extent was produced. When the circuit was made through the iron wire itself, a similar expansion was produced, and to about the same degree, as when under the influence of the helix. The total expansion, caused by the heating power of the current, was

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in this instance $\frac{1}{50}$ of an inch. The sudden expansion was much greater than when copper wire is employed; and is, I think, shown to be distinct from that resulting from the calorific influence of the current; from the fact that, when the current has developed the total expansion, which its heating power is capable of producing, if the circuit is interrupted, a similar sudden contraction takes place; and if, whilst the wire is still in its heated and expanded condition, the circuit is again completed, a sudden expansion also takes place, fully equal to that effected in the first instance. If, whilst under the influence of the helix, a voltaic current is sent through the central iron wire, the sudden expansive action is still produced in a somewhat greater degree; and also, when the reverse is effected, by transmitting through the helix a current at the time when the central iron wire is traversed by a continuous electric current.

With an iron wire of similar size, a remarkable difference was observed, after the wire had been well annealed. When subjected to the influence, scarcely any expansion seemed to be produced; and, after the circuit had been completed a few times in succession, no effect was observable, the index remaining motionless, though the wire elicited the usual tones as in other experiments. The action of the helix was now suspended; and the circuit completed through the iron wire itself, when a sudden and considerable expansion was produced of $\frac{1}{1000}$ of an inch. After interrupting the current a few times in succession, this sudden expansive action was also considerably diminished; but did not entirely disappear as in the former instance. If this current is now removed, and the iron wire again submitted to the influence of the helix, a considerable expansion is suddenly produced as in the last experiment, a sudden contraction also being shown when the circuit is interrupted; but, after this first effect, the helix seems to have no further influence in producing expansion, until a voltaic current is again sent through the substance of the iron wire. If a continuous current traverses the central iron wire, and a discontinuous circuit is established in the helix, a series of similar expansions and contractions is produced; but, as soon as the continuous current is interrupted, then the helix produces no further visible effect. I have tried this experiment with wires of different sizes, and with the same result, if the iron wire is not more than $\frac{1}{16}$ of an inch in diameter, and has been well annealed, and not exposed to any twisting or bending after such annealing. It would appear from these experiments, that the action of the helix impresses an opposite condition

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on the particles of the iron wire to that which the direct action of the voltaic current produces; and that the one action has a tendency to reverse the position of the molecules, which the effect of the other causes them to assume. M. De la Rive, in his very elaborate series of experiments on the vibrations of voltaic conductors, &c.,* has been led to form a similar conclusion, from observing that a distinct and peculiar sound is emitted, when an electric current and the magnetic helix were caused to act on the same wire. These results also tend to show that, when the iron is in a hard or unannealed condition, the molecules have a certain elasticity, that enables them to assume the position which they possessed previous to the action of the current or helix; whilst this position of the molecules in the unannealed wire is of a nature to prevent the wire from eliciting those vibrations that the current and helix excite; which position also is of a character to enable the wire to retain that degree of permanent magnetism, a property which such bodies are known to possess, in a certain degree, in common with steel.

I have repeated a few of those experiments which seemed to show that *hard* steel elicited no sound or vibration when exposed to the influence of the helix and find the results before indicated to be correct. This experiment affords a good method of discovering the value of steel bars which, when of good quality of *cast* steel and rendered quite hard in the usual way, emit no sound whatever when submitted within the helix to the influence of a powerful battery. When the bar is tempered or softened in the smallest degree, the usual tones begin to be elicited, increasing as the bar is softened, until, when rendered quite soft, the sounds become nearly as loud and distinct as those proceeding from bars of iron.

After a number of failures, I have succeeded in eliciting distinct intonations from an iron wire, by means of the discharge of the Leyden jar. To effect this, it is necessary to use a considerable extent of coated surface, highly charged; and to insulate carefully the wire, so as to prevent the charge from being dissipated in other directions; and it is essential that the battery should be discharged in such a manner as to remove from the apartment the sound of the explosion. When the discharge is made immediately through the metallic conductors, no sound is observed; but, when a piece of damp string or twine is interposed, a feeble sound is elicited, not much resembling the tones produced

* Vide *Elect. Mag.* Vol. II., p. 168.

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by the voltaic current and helix; but more like the sound observed when the wire is subjected to a slight mechanical disturbance. The principal difficulty to the success of the experiment arises from the degree of nicety required in the amount of humidity communicated to the interposed string; which, if very wet, appears to conduct the discharge too readily; and best succeeds when as little moisture is imparted as is sufficient to effect the discharge in a moderately short space of time. The iron wire which I employed was of the smallest size that I could procure, and was subjected to a slight degree of tension in connection with a sounding board. This result is chiefly interesting, as serving to confirm the experiments of M. Peltier,* which show that the magnetism produced in iron bars, by the electric or lightning discharge, is caused by the percussive or vibratory effect coercing the magnetism already induced in such bar.

The occurrence of these vibrations in metallic conductors naturally suggests the supposition, that a mechanical vibration or molecular disturbance may probably produce an electric current, and various attempts had been made by Matteucci, Ermann, and others, to effect this, by causing metallic rods to vibrate; but these experiments seem to have produced no decided result. Mr. Sullivan, however, has stated† that he has obtained deflections of the galvanometer, by causing rods or wires of two different metals to vibrate by mechanical means. I have tried a number of experiments on bars of different metals, and some of considerable size, and also arranged, as in Mr. Sullivan's method, various bars soldered or screwed together; but, when the necessary precautions were taken to prevent the interference of heat and of magnetism, when iron was employed, I could not obtain any deviation of the galvanometer needle. Since the appearance of Mr. Sullivan's communication, I have repeated several experiments with bars resembling those employed by him; but I have not succeeded in obtaining the result indicated. I do not wish to throw the least doubt on the experiments in question, as they affirm a result which appears exceedingly probable; but merely to state that, with the apparatus which I have used, no decided effect has been obtained, in order to lead others to repeat and verify the interesting experiments in question. Probably the galvanometer which I have employed may not be sufficiently sensible to indicate currents so feeble, though it is such that the heat of the hand, when brought within a

* Vide *Elect. Mag.* Vol. I. p. 562. † Ibid. Vol. II. p 193.

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considerable distance of any of the metallic junctions, produces a deviation of several degrees.

I have been more successful in obtaining indications of an electric current from molecular disturbance, by means of torsion, applied to bars of different metals, though the results are not yet sufficiently regular and decisive as to allow of much dependence being placed upon them. The bar under experiment was retained in a fixed position by inserting one end into an upright piece of brass, where it was retained by a screw; a lever also was affixed to the opposite end in a similar manner. When torsion is applied, if that part of the apparatus which detains and twists the bar is included in the circuit with the galvanometer, a deflection is almost always produced; but this arises from what M. Ermann designates the tribo-thermic electric current* being evolved, apparently by the friction of the metals at the point of junction. This effect is of course essentially different from a current produced by internal vibration, or molecular disturbance of the bar itself; and, consequently, that part of the apparatus must be excluded from the circuit. When this is effected a deflection of the needle is frequently produced by the application of torsion, and with greater effect generally when the metal is in a tubular form. I have obtained the best results with brass tubes from $\frac{1}{4}$ to $\frac{1}{2}$ inch in diameter, and occasionally with compound bars soldered together in the middle. The direction of the current is not well determined, being sometimes in one direction, and at others in the opposite; but frequently, when a deflection has been obtained by torsion in one direction, the sudden reversal of torsion has produced a deviation in the opposite direction. The direction of the bar, with respect to the magnetic meridian, does not appear to have had any particular influence on the experiment; but, in general, I preferred to place the bar in a position perpendicular to that of the dipping needle, so as to neutralize the effect of the induced magnetism from any iron in the composition of the bar or tube.

I was led, in these experiments, to make a few observations on the magnetism developed by torsion in iron rods, some of which results exhibit a considerable degree of complexity and irregularity. One of the effects noticed, however, seems to have a relation to some of the experiments before considered, as showing a connection between magnetic and molecular arrangement.

The iron bars experimented upon were about 4 feet in length, and from $\frac{1}{2}$ to $\frac{3}{4}$ inch in diameter, and fixed at one

* Vide *Elect. Mag.*, Vol. II., p. 140.

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end in a small brass frame, moveable to any position required; the opposite end being also fixed so as to retain it in one position, but allowing of motion on its axis when torsion was applied by a lever attached to the same end of the iron bar. The lever was so constructed as to admit of being fixed in any particular position, the degree of torsion being measured by an index and scale. The experiments were not designed to communicate any permanent degree of twisting to the bars, which in general returned the index nearly to the same point from which it started. A suspended magnetic needle, with the usual scale, was placed near to each end of the bar to indicate the extent of magnetic energy developed in the iron.

The degree of magnetism excited by torsion in the iron bar of course increases as the bar approaches to a position parallel to the magnetic needle; but it is worthy of observation, that the degree of magnetism communicated to the bar by torsion is always much greater than that which is induced in the bar merely by its position in relation to the earth, the act of twisting not only coercitating the magnetism already induced in the bar, but causing it, by apparently increasing its receptive capacity of induction, to retain a still higher degree of magnetic energy. This fact, of course, has been noticed from the first experiments on the effects of torsion; but it appears to me to require some further elucidation. However, when a degree of magnetism is thus excited in an iron bar by torsion, the effect is not fully developed until the torsonial force is removed; and when the bar has received or exhibits the greatest amount of magnetic force of which it is capable by this method, if the torsion is again applied, a certain amount of the magnetic force is apparently suspended during such application, being again restored when the torsional effect is removed. By thus alternately applying and suspending the torsional force to an iron bar, a magnetic needle in its vicinity may be kept in a state of rotation so long as such application is continued. I have tried this experiment with various iron bars, both hard and annealed, and with a nearly uniform result, from which the apparently paradoxical effect is manifested,—that the application of the same force in one instance increases, whilst in the other it diminishes the degree of magnetic force rendered evident. Apologising for the extent to which I have prolonged these, I fear, tedious remarks,

I am, sir, your obedient servant,

March 6, 1846. WM. BEATSON.

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CXLIX. *Voltaic Regulator.* By Mr. J. W. Starr.

In experiments where long continued currents of electricity are required, it is of great importance to have the means of regulating the battery, so as to avoid the diminution of power which occurs when the electrolyte begins to lose its conductibility from the formation of sulphate of zinc. In such cases I have found the simple apparatus here described to answer the purpose. A soft iron magnet of the U form has a hole drilled through each pole in the same direction so as to allow an iron rod about 0·2 inch diam. to pass freely through them. This rod is the armature of the magnet. It may be about 12 or 14 inches in length, and should be smooth and well polished. The magnet is wound with stout copper wire in the usual manner, except that a short space is left uncovered at the ends to admit the rod, and in order that this may have a vertical motion given to it, the magnet is fixed horizontally to an upright support. A small circular plate is screwed on the lower end of the rod to serve as a support for weights when a powerful current is to be employed. When the electro-magnet is included in the battery circuit, the iron rod is of course supported by the attractive power, and one of the plates of the battery if attached to it may be raised out of the liquid till the power of the magnet is barely sufficient to support the weight of the plate. It will remain at this height till the power of the current begins to decline, when the rod will gradually slip down and allow the plate to be immersed to a greater depth in the liquid. The increased resistance of the liquid is thus counterbalanced by increasing the area of its section, and a continuous and steady current is obtained. The iron rod should be equally annealed throughout its entire length, and the interior of the holes in the magnet through which it passes should be lined with thin paper.

In working with Smee's battery, I find the following to be a very convenient way of keeping up a regular supply of acid. A glass tube, which may be about 2 inches in length and 1 inch in diameter, is drawn out at one end to a fine point, so as to leave a very small orifice. A sufficient quantity of acid being poured into it, it is suspended in the acidulated water contained in the battery cell. It may be furnished with a projecting piece at the top, fitting over the edge of the cell, like the pierced boxes sometimes used for holding a supply of sulphate of copper. The size of the orifice is easily regulated by softening the point of the tube

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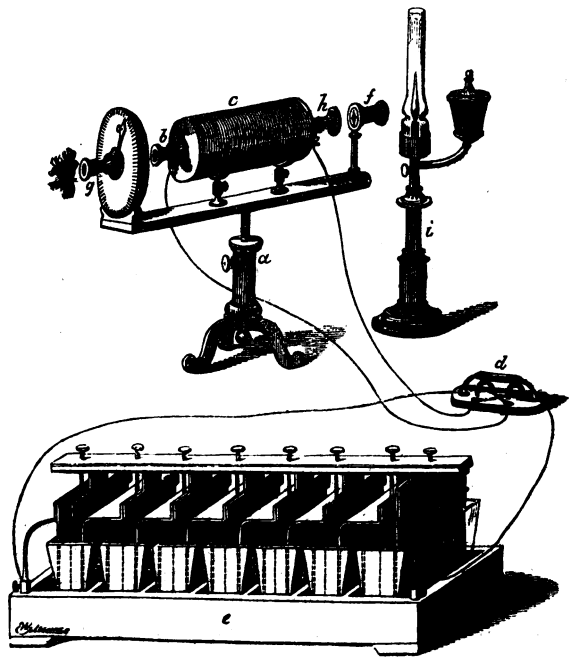
in the flame of a lamp. By this means a gradual and steady supply of acid may be kept up for several days, or even weeks, and the local action which occurs when the zinc is plentifully supplied with acid in the usual manner is avoided.

CL. *Professor Böttger's Arrangement for the Performance of Farrday's recent Experiment.* Translated from Böttger's *Beiträge zur Physik et Chemie Drittes Heft.* P. 1; and communicated by W. G. Lettsom, Esq.

(Extract.)

I here without hesitation lay before my colleagues, in as concise a manner as I can, the method that I have so successfully adopted, I hope without unduly anticipating the further communications which we speedily look for in detail from the discoverer, and without in any way infringing upon what is due to him. I confine myself, therefore, to a plain description of the apparatus as employed by me and to the



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method to which I had recourse, in order, *by means of a powerful electro-magnet,* to render a ray of polarised light visible, and make it disappear, and to impart rotation to its plane of polarisation.

For this purpose I make use of the apparatus as ordinarily constructed for the examination of the circular polarisation of fluids, *a* supporting a pair of achromatic Nicol's prisms, placed horizontally, *g* and *f*. Between these there is placed a brass tube some two or three lines in diameter, and from 6 to 8 inches long, closed at both ends by plates of glass *b* and *h*. The length of the tube in my apparatus is 57 French inches. I fill the tube with any double-refracting fluid; for instance, transparent syrup, tartaric acid, oil of turpentine, &c., &c. The two fluids that I most frequently used, were a solution of sugar candy, consisting of one-third candy and two-thirds water, or common oil of turpentine.

This being done, the tube is to be placed in the axis of a *hollow* helix, which is lined throughout its entire length with a thin *cylinder of sheet iron, c*. The helix that I employed was $5\frac{1}{4}$ inches long, the aperture traversing it was $1\frac{1}{2}$ in diameter, and it consisted of five turns of thick copper wire carefully insulated. The projecting terminals of the helix admitted of their being brought, by means of a commutator *d*, into connection with the poles of a Grove's battery of six or seven pairs, *e*.

Now, on letting the light of an Argand lamp, *i*, pass through the hindermost Nicol's prism, and thus causing a ray of polarised light to traverse the saccharine solution in *h, b*, it will be observed that a certain position may be given to the front moveable prism, *g*, in which the field is dark, (when I employed homogeneous *yellow* light, the rotation was, with the above sugar-candy solution, to the right, and amounted to 40°); but if now, by completing the circuit, we cause the galvanic current to traverse the helix *c* in such a manner that it enters the right-handed helix, where the polarised ray enters the saccharine solution—the longitudinal magnetic axis coinciding with axis of the ray—or in other words, the magnetic north pole being at *b*, and the south pole at *h*, there will instantly be indicated a rotation of the plane of polarisation to the *left*, amounting to 1° 15′, the field no longer remaining dark but becoming of a *reddish* hue, the phenomenon remaining constant as long as the circuit is closed.

On inverting the current, by means of the commutator, however, so that the north pole is brought to *h* and the

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south pole to *b*, we shall observe that the plane of polarisation is inverted to the right; and, as before, to the amount of 1° 15′, the field at the same time becoming of a *bluish green* tint. In using oil of turpentine, tartaric acid, syrup, and so forth, *whatever might be* the concentration of those fluids, it was found that the amount of rotation in the plane of polarisation, right or left, according to the direction of the current, was always *constant*, and, as has been stated, 1° 15′; and this, even when the number of the pairs of plates was diminished. The angle of rotation, on the contrary, appeared somewhat diminished upon shortening the length of the wire forming the helix, in other words, when the electro-magnetic force was reduced.

With a helix *without* a cylinder of sheet iron, the rotation of the plane of polarisation was likewise observable it is true, but only to a very limited extent. Even on approaching from above only *one* pole of the helix in question, when provided with a *massive* iron rod in its centre, I noticed the production of a limited rotation. For instance, upon bringing that end of the iron rod which, upon the closing of the circuit indicated *north polarity* towards that end of the brass tube where the polarised ray enters, there followed an extremely limited rotation to the *right;* on the approach of the *south pole,* there was indicated an extremely limited rotation to the *left.* A directly opposite result followed when *one* pole of the electro-magnet was brought near the *other* end of the ray of light. If, for instance, either above or at the side, the *north pole* was brought near to that end of the tube marked *b*, there was observed a very limited rotation to the *left;* on approaching the *south pole*, to the *right.* The diamagnetic glass containing borate of lead, of which Faraday makes special use, can therefore, as we see from the above, be perfectly replaced by any fluid possessing double refraction.

CLI. *Upon the Indications of a Modification manifested by Polarized Light in the Molecules of Fluid and Solid Bodies when under the Influence of a Powerful Electro-magnet.* By Dr. Bottger. Translated from Bottger's *Beiträge zur Physik et Chemie Drittes Heft.* P. 127; and communicated by W. G. Lettsom, Esq.

Faraday's interesting discovery, the only account of which, up to the present time, is to be found in the indistinct and imperfect statements contained in newspapers, and of which

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I spoke in the earlier portion of the present series of papers, has latterly occupied my attention so entirely, that I shall, I hope, be pardoned if, in the absence of any direct and accurate account on the part of the discoverer himself, I here make known in as concise a manner as I can, certain experiments of my own relative to this subject, and from which, as it appears to me, we are justified in coming to the conclusion, that a magnet or an electro-magnet *does not directly affect a ray of polarized light;* but rather, that it is affected by magnetism only in consequence of its *modifying in some way or other, with which we are at present quite unacquainted, the medium, or the ponderable substance* traversed by the light. There are but two methods by which as yet we are able to form anything like a correct notion with regard to the internal structure of bodies; namely, with respect to substances crystallising in *regular forms*, either transparent or opaque from their cleavage; and with respect to *amorphous* transparent substances, whether solid or fluid, from certain modifications which they cause in the transmission of *light*. This latter method has rendered us far more intimately acquainted with the essential construction of bodies than the former one; and has, in fact, revealed to us phenomena dependent upon the form of their ultimate atoms, the existence of which we could scarcely, by any other means, have been led to suspect. And there is nothing that appears so likely, in due course of time, to furnish us with clearer notions with regard to the essential nature and molecular condition of matter, speaking in a general sense, than Faraday's recent discovery, from which the most important deductions may be anticipated.

From all the experiments that I have hitherto instituted, it appears completely established that simple magnetism does *not* directly affect either an *ordinary* or a *polarised* ray; but rather that Faraday's modification of the light is altogether attributable to a certain modification wrought instantaneously by magnetism, in the position of the ultimate atoms of the substance, consequently that magnetism must evidently exert an action upon substances, with respect to which we hitherto held that power to be inert. I likewise draw this further inference from my experiments, viz., that substances hitherto looked upon as indifferent to magnetism, for instance water, alcohol, glass, and so forth, become distinctly possessed of *bipolarity* by the approach of a powerful magnet. In other words, they indicate polarity in *this* sense, viz., that on approaching the *north pole*, for instance, towards *that end* of any transparent body, of a

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certain length, at which a polarized ray *enters*, the plane of polarization indicates a rotation to the *right;* whereas on the approach of the *south pole* to the *same end*, the rotation is to the *left*. If the approach is made towards the *opposite* end of the transparent body, that is to say, when the corresponding poles are applied to that end thereof where the ray emerges, there is a rotation in a contrary sense. Lastly, when either pole is brought near the *centre* of the body in question, I *could not perceive any indications of a rotation* either to the right or left.

Fluids, speaking generally, appear to be *more susceptible* of the magnetic influence than solid bodies are. I have not hitherto, from the want of suitable apparatus, been able to ascertain whether different vapours and gases manifest a similar action when traversed by a ray of polarized light; but as I now have such an instrument at my disposal, I will not fail in due time to give an account of the results at which I arrive.

At the conclusion of the former article upon this subject, I stated *that every fluid possessing double refraction* is applicable to the performance of Faraday's experiment. I may now add that, not only bodies possessing *single* and *double* refraction, but also those which impart *no circular polarization* whatever to light, become *manifestly endued* with this property the instant they are inserted in an excited hollow helix of several turns of wire, and which is lined with a cylinder of sheet iron. The substances with which I experimented upon this point were, among others, distilled water, absolute alcohol, ether, sulphuret of carbon, aqueous solutions of saltpetre, of ferrocyanide of potassium, of subsulphite of potash, of sulphate of iron. I further experimented with a common glass cane, 6 inches long, and polished at both ends (it was of white soda glass), as also with a cane of similar dimensions, which had been cooled, and which showed the well-known figures in polarized light. In all the substances here mentioned, the rotation of the plane of polarization takes place in the sense described in the former paper, that is to say, its direction is altogether dependent *upon the position of the poles of the electro-magnet.* When using the substances here named, the phenomena are not as marked as when bodies naturally possessing circular polarization are employed; as for instance, a solution of sugar, oil of turpentine, an aqueous solution of tartaric acid, and so forth (inasmuch as, with ordinary non-monochromatic lamplight, the complementary colours are not visible in the field), but nevertheless the effect is so distinctly marked that

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on the approach of a very powerful electro-magnet, I uniformly obtained a rotation amounting to 2° 30′.

For instance, when I filled the brass tube 7·5 French inches long (marked *b*, *h*, in the former paper), with *distilled water* instead of the solution of sugar, and placed the axis of the analysing prism, *g*, perpendicular to that of the hindermost prism, *f*, so that, in other words, the index was at zero, and the field dark, I found, on completing the circuit, in such a manner that the south pole of the hollow electro-magnet was at *b*, and the south pole at *h*, that the field was instantly rendered bright, and remained so during the time that the circuit was closed, the rotation of the plane of polarization amounting exactly to 2°·5, and being to the left. When I made the polarized ray that had traversed the water in the tube pass through a like distance of air, and placed the helix in front of the brass tube, *b*, *h*, that is to say, between the analysing prism, *g*, and the extremity, *b*, *I did not observe the slightest rotation* of the plane of polarization upon completing the circuit; but the phenomenon was instantly brought about directly I pushed the hollow helix back again over the column of water contained in the brass tube.

If, therefore, a ray of polarized light, instead of being made to traverse a transparent solid or fluid body is passed through an equal distance of atmospheric air, that is surrounded, as one may say, with an electro-magnetic helix, no rotation of the plane of polarization is observable on the completion of the circuit.

SCIENTIFIC MEETINGS.

ROYAL SOCIETY.

Jan. 8th and 15th. — The following paper was read: — "*Experimental Researches in Electricity*," by Mr. Faraday. Twentieth Series. Section 26th. "*On New Magnetic Actions; and on the Magnetic Condition of all Matter.*"

The following is the order in which the several divisions of the subject treated of in this section of the author's researches in electricity succeed one another: — 1. Apparatus required. 2. Action of magnets on heavy glass. 3. Action of magnets on other substances acting magnetically on light. 4. Action of magnets on the metals generally. 5. Action of magnets on the magnetic metals and their compounds. 6. Action of magnets on air and gases. 7. General considerations.

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In giving an account of the contents of this paper, any attempt to follow the track of the author in the precise order in which he relates the consecutive steps of his progress in this new path of discovery, would fail of accomplishing its object: for, by adhering to such a course, it would scarcely be possible to comprise within the requisite limits of an abstract the substance of a memoir extending, as the present one does, to so great a length, and of which so large a portion is occupied with minute and circumstantial details of experiments; or to succeed in conveying any clear and distinct idea of the extraordinary law of nature brought to light by the author, and of the important conclusions which he has deduced.

One of the simplest forms of experiment in which the operation of this newly-discovered law of magnetic action is manifested, is the following:—A bar of glass, composed of silicated borate of lead, two inches in length, and half an inch in width and in thickness, is suspended at its centre by a long thread, formed of several fibres of silk cocoon, so as to turn freely, by the slightest force, in a horizontal plane, and is secured from the agitation of currents of air by being enclosed in a glass jar. The two poles of a powerful electro-magnet are placed one on each side of the glass bar, so that the centre of the bar shall be in the line connecting the poles, which is the line of magnetic force. If, previous to the establishment of the magnetic action, the position of the bar be such that its axis is inclined at half a right angle to that line, then, on completing the circuit of the battery so as to bring the magnetic power into operation, the bar will turn so as to take a position at right angles to the same line; and, if disturbed, will return to that position. A bar of bismuth, substituted for the glass bar, exhibits the same phenomenon, but in a still more marked manner. It is well known that a bar of iron, placed in the same circumstances, takes a position coincident with the direction of the magnetic forces, and therefore at right angles with the position taken by the bar of bismuth subjected to the same influence. These two directions are termed by the author *axial* and *equatorial;* the former being that taken by the iron, and the latter that taken by the bismuth.

Thus it appears that different bodies are acted upon by the magnetic forces in two different and opposite modes; and they may accordingly be arranged in two classes; the one, of which iron is the type, constituting those usually denominated *magnetics;* the other, of which bismuth may be taken as a type, obeying a contrary law, and therefore coming under the generic application of *diamagnetics*. The author has examined a vast variety of substances, both simple and compound, and in a solid, liquid, or gaseous form, with a view to ascertain their respective places and relative order with reference to this classification. The number of simple bodies which belong to the class of magnetics is extremely limited, consisting only of iron, which possesses the magnetic property in an eminent degree, nickel, cobalt, manganese, chromium,

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cerium, titanium, palladium, platinum, and osmium. All other bodies, when either solid or liquid, are diamagnetic; that is, obey the same law, with regard to magnetic action, as bismuth, but with various degrees of intensity: arsenic is one of those that give the feeblest indications of possessing this property. The following exhibit it in increasing degrees, according to the order in which they are here enumerated; namely, ether, alcohol, gold, water, mercury, flint glass, tin, lead, zinc, antimony, phosphorus, bismuth. On the other hand, no gaseous body of any kind, or in any state of rarefaction or condensation, affords the slightest trace of being affected by magnetic forces. Gases may therefore be considered as occupying the neutral point in the magnetic scale, intermediate between magnetic and diamagnetic bodies.

The magnetic properties of compound bodies depend on those of their elements; and the bodies are rendered either magnetic or diamagnetic, according to the predominance of one or other of these conditions among their constituent parts. Thus iron is found to retain its magnetic power when it has entered into combination with other bodies of the diamagnetic class; the two forces acting in opposition to one another, and the resulting effect being only that due to the difference in their power. Hence the oxides and the salts of iron are still in a certain degree magnetic, and the latter even when they are held in solution by water; but the water may be present in such a proportion as that neither shall prevail; and the solution, as far as respects its magnetic properties, will then be exactly neutralized. These saline solutions, prepared of various degrees of strength, also afford a convenient method of comparing the relative degrees of force, both magnetic and diamagnetic, of different bodies, whether solid or fluid, but more especially the latter, as they admit of the body under examination being suspended in another liquid, when its position of equilibrium will indicate which of the two substances has the strongest magnetic power.

In one respect, indeed, the diamagnetic action presents a remarkable contrast with the magnetic; and the difference is not merely one of degree, but of kind. The magnetism of iron and other magnetics is characterized by polarity; that of diamagnetics is devoid of any trace of polarity; the particles of two bodies of the latter class, when jointly under the influence of the magnetic forces, manifesting towards each other no action whatever, either of attraction or repulsion. It has long been known that the magnetism of iron is impaired by heat; and it has been generally believed that a certain degree of heat destroys it entirely. The author finds, however, that this opinion is not correct; for he shows that, by applying more powerful tests than those which had been formerly confided in, iron, nickel, and cobalt, however high their temperature may be raised, still retain a certain amount of magnetic power, of the same character as that which they ordinarily possess. From the different temperatures at which the

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magnetic metals appear to lose their peculiar power, it had formerly been surmised by the author that all the metals would probably be found to possess the same character of magnetism, if their temperature could be lowered sufficiently: but the results of the present investigation have convinced him that this is not the case; for bismuth, tin, &c., are in a condition very different from that of heated iron, nickel, or cobalt.

The magnetic phenomena presented by copper and a few other metals are of a peculiar character, differing exceedingly from those exhibited by either iron or bismuth, in consequence of their being complicated with other agencies, arising from the gradual acquisition and loss of magnetic power by the iron core of the electro-magnet, the great conducting power of copper for electric currents, and its susceptibility of being acted upon by induced currents of magneto-electricity, as described by the author in the first and second series of these researches. The resulting phenomena are to all appearance exceedingly singular and anomalous, and would seem to be explicable only on the principles referred to by the author.

Pursuing his inductive inquiries with a view to discover the primary law of magnetic action from which the general phenomena result, the author noticed the modifications produced by different forms given to the bodies subjected to experiment. In order that these bodies may set either axially or equatorially, it is necessary that their section, with reference to the plane of revolution, be of an elongated shape: when in the form of a cube or sphere they have no disposition to turn in any direction: but the whole mass, if magnetic, is attracted towards either magnetic pole; if diamagnetic, is repelled from them. Substances divided into minute fragments, or reduced to a fine powder, obey the same law as the aggregate masses, moving in lines, which may be termed *diamagnetic curves*, in contradistinction to the ordinary magnetic curves, which they everywhere intersect at right angles. These movements may be beautifully seen by sprinkling bismuth in very fine powder on paper, and tapping on the paper while subjected to the action of a magnet.

The whole of these facts, when carefully considered, are resolvable, by induction, into the general and simple law, that while every particle of a magnetic body is attracted, every particle of a diamagnetic body is repelled, by either pole of a magnet. These forces continue to be exerted as long as the magnetic power is sustained, and immediately cease on the cessation of that power. Thus do these two modes of action stand in the same general antithetical relation to one another as the positive and negative conditions of electricity, the northern and southern polarities of ordinary magnetism, or the lines of electric and of magnetic force in magneto-electricity. Of these phenomena, the diamagnetic are the most important, from their extending largely, and in a new direction, that character of duality which the magnetic force was

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already known, in a certain degree, to possess. All matter, indeed, appears to be subject to the magnetic force as universally as it is to the gravitating, the electric, the cohesive, and the chemical forces. Small as the magnetic force appears to be in the limited field of our experiments, yet when estimated by its dynamic effects on masses of matter, it is found to be vastly more energetic than even the mighty power of gravitation, which binds together the whole universe: and there can be no doubt that it acts a most important part in nature, and conduces to some great purpose of utility to the system of the earth and of its inhabitants.

Towards the conclusion of the paper, the author enters on theoretical considerations suggested to him by the facts thus brought to light. An explanation of all the motions and other dynamic phenomena consequent on the action of magnets on diamagnetic bodies might, he thinks, be offered on the supposition that magnetic induction causes in them a state the reverse of that which it produces in magnetic matter; that is, if a particle of each kind of matter were placed in the magnetic field, both would become magnetic, and each would have its axis parallel to the resultant of magnetic force passing through it; but the particle of magnetic matter would have its north and south poles opposite to, or facing, the contrary poles of the inducing magnet; whereas, with the diamagnetic particles, the reverse would obtain; and hence there would result in the one substance approximation, in the other recession. On Ampère's theory, this view would be equivalent to the supposition that, as currents are induced in iron and magnetics parallel to those existing in the inducing magnet or battery-wire, so, in bismuth and other diamagnetics, the currents induced are in the contrary direction. As far as experiment yet bears upon such a notion, the inductive effects on masses of magnetic and diamagnetic metals are the same.

ROYAL INSTITUTION. 1846.

January 23d and March 6th.—On the former of these evenings, Professor Faraday illustrated the newly discovered relations between light and magnetism; and on the latter, the extraordinary fact, that all bodies are acted upon by magnets, a few, such as iron, &c., being attracted, and all the rest repelled. A report of the one subject has been already given in the *Electrical Magazine*, No. 11; and of the other, a report appears in the pages of the present number. We need not, therefore, repeat it here, especially as, in our next, we shall give a full analysis of the two memoirs, which, by that time, will have been made public in a printed form. The doors of the Institution were literally besieged on each evening, and fortunate were they who were not included in the number of those who could not find an unoccupied spot in the lecture-theatre. The action of magnetism on polarized light was shown to the whole

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audience upon a screen placed behind the professor. The polarizing apparatus was arranged by Mr. Darker. The second illustration out-Faradied Faraday. It was "got up" better than even *he* is wont to do, by means of accurately measured cocoon threads, carefully-adjusted stirrups, and glass shades, &c. The delicate experiment of giving a directive tendency to such ordinary bodies as wood, paper, air, &c., were made manifest to all present. The powerful magnet was placed beneath the table, poles upward, and these appeared just above the surface of the table, and in front of the lecturer, so that he had his power readily at command. And what a power!—But of this more anon, when we have his paper before us, and are able to give the full description of his apparatus. We need scarcely say that, among the visitors, were all the leading men of science in London, each anxious to devour the extraordinary truths which this persevering philosopher has now brought to light. Homer is said sometimes to have slept; Faraday may theoretically be said *never* to sleep. While other men are beating about the bush, and thinking that perhaps there may be something hidden within, he moves onwards—he dives into the bush—he moves away the thorns and the briars, and brings to light things which were just as accessible to others had they evinced the painstaking and the perseverance that he has. Many were on the threshold of these discoveries; but there they stood. Faraday went quietly onward, and he has gathered laurels that will not soon be taken from him. In common with all the lovers of physical science, we look anxiously for the printed papers, and only regret that we have them not in time to report upon in this Number.

ROYAL ACADEMY OF SCIENCES, PARIS. 1845.

(*Continued from p.* 224.)

Nov. 24.—Mr. DUJARDIN addresses a note, relative to an *electro-magnetic battery*, which he has devised, and which he thinks would produce very powerful induced currents; he has not constructed the apparatus on account of the expense; but thinks it would be useful in electro-chemistry and in electro-telegraphy. No description is given.

Dec. 8.—Mr. Phillips, of London, addressed a note, in order to show that for curing certain *aneurismal tumours* by means of galvano-puncture, he had made experiments several years anterior to those of M. Petrequin. (Vide *Elec. Mag.*, vol. ii. p. 206; No. 99.)

Dec. 15.—M. PERROT claims priority in *electro-chemical gilding and plating*. He says, "Two months before Mr. Elkington, and ten before M. de Ruolz, had proposed even a partial application of the new electro-chemical processes, I had arrived at results imme-

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diately applicable in the arts. The processes, contained in the patents taken out subsequently are the reproduction, either of processes that I had myself employed, or of those which MM. Smee, Lourget, Pechiney, Soul, &c., had made public."

Dec. 22.—*Application of Metals on Metals.*—M. CHRISTOFLE, in reply to M. Perrot.—In the absence of Mr. Elkington, M. Christofle says, that if in August, 1840, prior to Mr. Elkington's patent of September, 1840, M. Perrot had gilt or plated or platinated, by means of the cyanides, he had not published his discovery: and when he afterwards sent specimens to the Academy, he had not stated that he used the cyanides. With respect to the patented processes being but the reproduction of methods described by Smee and others: the single answer is, that "the publication made by the persons to whom M. Perrot alludes are posterior to the 29th of September, the date of Elkington and de Ruolz's patents."

Dec. 29.—*On adhering Metallic Precipitations*, by M. de RUOLZ.—The author alludes to the discoveries, and to their priority, in reference to the claims of M. Perrot; he also says, in one of his letters:—

"I have the honour to state to you that, for three months, and before there was any question about M. Perrot's reclamation, I have obtained four new processes; one particularly suitable for gilding silver; the other to gilding bronze; and the object of the third is plating.

"These liquors present notable advantages over those hitherto known, by their excessively low price, and by the innocence of the products of which they are composed; by the complete absence of all smell, and the means they present of suppressing without inconvenience the employment of soluble ozade, which on a large scale in the arts, absorb a considerable capital.

"The object of the fourth is platinating; and it gives the means of depositing platinum in much thicker layers than has hitherto been possible.

"Having to take measures that no private interest shall suffer from the publication of these processes, I placed them with their date in the hands of M. Chevreul, who has kindly consented to accept the deposit, until the approaching day, when I shall have the honour of communicating them to the Academy."

Jan. 12, 1846.—In allusion to Mr. Grove's proposition of using a *voltaic light for certain illumination*, and De la Rive's idea of employing it in mines,* a member remarks that he cannot help recognising M. BOUSSINGAULT as the first to publish the idea after having practically applied it in mines.

* Vide *Elect. Mag.*, Vol. II., p. 222, No. 11.

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On the Conductibility of the Earth for the Electric Current. By M. MATTEUCCI. (*Vide supra*, p. 288.)

Jan. 19.—*New Relations between Light, Electricity, and Magnetism.* By Prof. FARADAY. (*Vide supra*, p. 259.)

Jan. 26.—*Note on Faraday's New Experiments.* By M. POUILLET. (*Vide supra*, p. 261.)

Action of Magnetism on all Bodies. M. BECQUEREL. (*Vide supra*, p. 272.)

Apparatus for Experiments on Light and Electricity. M. DESPRETZ. (*Vide supra*, p. 274.)

Flash of Lightning on a House furnished with a Lightning Conductor. M. de CARVILLE.

"On the morning of Dec. 20, 1845, during a heavy hail shower, *a fire-ball was seen to bifurcate in the vertical of the lightning conductor* placed at the centre of the chateau (of Boisyvon, near Vire) 9.1 metres in length above the top of the roof. The electric fluid immediately produced great havoc on both sides of the chateau, at 9 metres distance from the conductor. In the points where the electricity reached the earth, several persons perceived *as it were a large tube of fire rolling on the ground.*"

"The properties of *ball-electricity* are little known. It is not even known whether Franklin's invention is a security against lightning when it assumes this form; but in the present case there is no need to go so far to find the cause of the inefficiency of the lightning conductor of the chateau of Boisyvon; the following passage of the note explains the phenomenon.

"The conductor of the lightning-rod descends into the ground by a drain 0·11 metres square at the surface of the said ground, and 0.20 metres at the moment when it enlarges, *and forms a walled reservoir of about a metre in diameter.* The whole was filled with carbon.

"*A walled reservoir* speaks volumes."

Feb. 2. — *Construction of a very Powerful Magnet.* By M. BABINET. (*Vide supra*, p. 290.)

M. LOUYET, Professor of Chemistry in the Brussel's Museum of Industry, writes to say that he had proposed the *voltaic light for mines* long before M. Boussingault. He quotes the following from an article of his, published Oct. 26, 1836, in the *Courrier Belge.*

"Since we are on the chapter of coal-mines, it seems to us that here is the place to express our astonishment at seeing that one of the most beautiful discoveries of modern science has not been rendered available for the illumination of the galleries. We would speak of the incandescence of charcoal produced in vacuum by means of a voltaic pile . . . . There would not be the least fear of danger, since the focus of light would be completely insulated from the external air."

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M. BOUSSINGAULT grants that he did not entertain the idea of applying the voltaic light for the illumination of mines, until June, 1845, after an accident that had happened in his mines; he thinks it, however, his duty to add that he did not content himself with *proposing* this means, but that he actually *employed* it to illuminate without danger an atmosphere of a most explosive character.

NEW BOOKS.

Part I. Philosophical Transactions for 1846, *containing Faraday on Light and Magnetism, &c.*, 7*s.* 6*d.*—*The American Electro-Magnetic Telegraph, &c.*, 81 *engravings. Alfred Vail, Philadelphia.*—*Reichenbach's Researches on Magnetism, &c., Gregory's translation*, 3*s.*

NOTICES OF EXPERIMENTS, &c.

Physiological Telegraph.—" In a very cleverly-written memoir, Professor Vorselman de Heer de Deventur put forth, in 1839, the singular proposition of employing, for the transmission of telegraphic signals and despatches, electric shocks, which might be propagated to very great distances by means of a voltaic pile or the apparatus for electro-magnetic induction. Mr. V. de H. constructed, for this purpose, at each station, a kind of finger-board composed of ten double keys. The clerk who transmits the signals, as well as the one who receives them, both keep their ten fingers on these keys. If the former presses down any two keys, he connects the battery with the conductor, and the other receives a shock in the two fingers that rest upon the two similar keys. In this manner, by means of suitable combinations, forty-five different signals may be given, a number more than sufficient for telegraphic wants. If it were necessary, in this system, to employ violent electric shocks, it must be entirely rejected, as the telegraphic duties must be regarded as a severe penalty. This is not the intention of the author, for powerful galvanic shocks may now be so reduced as to become nothing more than very light sensations. But setting aside these shocks, Mr. V. de H.'s idea, although perfectly well-grounded in a scientific point of view, presents great inconveniences in practice, and has not been cleverly executed. Notwithstanding this, I stand up in its defence, because I have had occasion to be convinced that advantages may be derived from it. The principle merely requires certain modifications. The number of conducting wires and fingers to be removed must be reduced to two. To obtain the two signs that are required as elements of combination, a single or a double stroke must be given with the key, which produces at the other station two sensations very distinct from each other. The thumb and fore-finger of the left hand are best suited for this office, because, whilst they rest upon the

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two plates of metal that are in good communication with each other, the right hand remains free, and may thus note down the signals at the very moment when they arrive. With a little practice, it is easy to distinguish a triple from a double blow. When the blow is quadruple, the sensations begin to be confused. In the experiments that I made last winter on the ice of the Neva, and in which the distance between the station was nine verstes (1,800 yards), which rendered the employment of telegraphic signals necessary, I derived a great advantage both in point of simplicity and in convenience of transport, in the employment of a physiological telegraph. This instrument possesses something really curious and mysterious. We feel ourselves, so to speak, in corporeal contact with the person with whom we correspond, and who may be at the distance of several miles. If the apparatus is suitably erected, we might, in the midst of a numerous society, both transmit and receive certain conventional signals without any of the persons present perceiving them."—*Jacobi's Memoir to the Imperial Academy*, Jan. 8, 1844.

Magnetism and Molecula Arrangement.—Several effects in thermography induced Mr. Hunt to think that magnetism had some influence over molecula arrangement; experiment confirmed his view. 1. He placed concentrated solution of nitrate of silver in a bent tube over the pole of a horse-shoe magnet; and similar solutions not under such influence. Crystallization commenced first in the former, and all the crystals arranged themselves at angles inclining toward the magnet; in the other tube the crystals were less dense, and were irregularly diffused. 2. That the cooling effect of the metal had no part in this, was shown by placing the second tube in connection with a mass of brass, similar comparative results were obtained. 3. When the solution was placed between the press and the magnet, the mass of crystal sprung from near the north pole, while three long crystals darted from the south pole and joined the others. 4. When the last experiment was repeated with the solutions on blackened glass, the currents of the one under magnetic influence presented a series of perfectly regular curved lines from the circumference to the centre; but no regular currents were observable in the other. 5. and 6. Needles suspended to the respective poles were allowed to dip in the solution, a crystalline pellicle formed and took the shape of the magnetic curves. 7. When the wires defended with sulphate of iron, the same general arrangement prevailed, crystallization commencing at the north pole. 8. and 9. So also with proto-nitrate of mercury. 10 and 11. On pouring strong warm solution of nitrate of mercury into a shallow trough placed over a powerful magnet, crystallization in beautifully regular curves immediately commenced. 12. On pouring nitrate of silver upon a copper plate within magnetic influence, and pouring it off after a minute, the reduced silver arranged itself in curves; they were evanescent on the impure copper of commerce; but semonal on electrotype copper. 13. By

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allowing such an experiment as the last to continue for a night, the oxidation of the copper surface was in curved lines. 14. Into weak solution of nitrate of silver, under magnetic influence, was poured weak solution of sulphate of iron, the precipitated silver was in curves. All which experiments "show that magnetism exerts a powerful influence on molecula arrangements, and that it regulates the direction of crystalline formations."—*Philosophical Magazine*, Jan. 1846.

Morse's Telegraph.—"The establishment of magnetic telegraphs, radiating from New York to the east, west, and south, to the extent of three or four hundred miles, so as to connect all the large cities of the Atlantic border with this metropolis, is now in a rapid state of progress. The line between this and Buffalo is under way, and so also is the one to Boston, so likewise that between this city and Washington, including all the intermediate points. There is also a line commenced between the city and Coney Islands. All these lines are conducted by associations of individuals, who derive their powers from the inventor, Professor Morse, now in Europe. It is supposed that the whole of them will be finished in the course of three or four months; and that the principal line between this and Washington will be completed in time to transmit the next message of the President to this city, and to enable the publishers to issue it simultaneously with the Washington papers. When completed, these united lines of telegraphic communication will embrace a territory of nearly 500 miles from south to north, and from east to west, including within its ramifications the metropolis, Washington, Buffalo, Boston, with all the intermediate cities, as part of the grand scheme of communication."—*New York Herald.*

Electro-Physiology.—"About the beginning of September, 1843, a small patch of fungus of a peculiar character, was observed to have commenced forming on the outside of the glass, near its lower rim. This substance having, when first seen, a gelatinous appearance, of a dark brown colour, by slow degrees extended itself round the lower rim of the glass, forming an irregular band or zone, half an inch in breadth, and throwing out numerous protuberances as it approached the positive side of the arrangement. On the 29th of November, in the same year, the following note relative to this singular production occurs among my memoranda; and, as I cannot otherwise better describe its mature appearance, I shall subjoin the extract:—"The substance of this fungus varies in colour from a light chocolate to that of a dark sanguineous red, and though formerly of a soft texture, it now offers considerable resistance. When viewed with an excellent pocket lens—the only sort of microscope that can be brought to bear upon it—a most singularly beautiful species of vegetation is seen to occupy its entire surface, presenting various shades of crimson, green, olive, and green inclining to yellow. In its general appearance, it at

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once suggests the idea of a magnificent forest, consisting of trees and flowering shrubs in miniature. In particular spots, fine, downy, needle-like spires occurs in vast multitudes, and these otherwise naked processes rising from the body of the fungus, are surmounted by what appear to be seed vessels in some instances, and irregular feathery tufts in others. This experiment was not designed with any reference to researches on the development of the electrical acari; but swarms of these creatures appeared incidental to its progress, and at the time the above note was made, many of them were inhabiting the miniature forest on the fungus, where they seemed to thrive amazingly, and to attain a larger size than any I have hitherto seen."—Mr. Weekes's *Letter in Sequel to Vestiges of the Natural History of the Creation.*

Aurora Borealis. — "After a heavy fall of rain on Sunday morning (Jan. 4, 1846), with a brisk southerly wind, about midday the rain ceased, and towards evening the wind, having abated, shifted to the north, the sky, at the same time, becoming gradually clearer, as the clouds were dispersed or dissipated. Till towards ten o'clock it was perfectly cloudless, the atmosphere being exceedingly clear, and the moon (then entering her second quarter) and stars shining very brightly, with a freezing temperature. About 10 h. 30 m. a broad nebulus auroral arch extended itself nearly across the heavens, from the N.N.E. in a S.S.W. direction, its northern extremity being at an altitude of from 15° to 20°; the arch ran along a meridian of from 60° to 70° with the western horizon, passing *through* the Polar star and Pleiades, about 10° or 15° south of which latter constellation it terminated. This broad fleecy belt was most luminous towards the centre, and became gradually fainter towards its limbs. By degrees the intensity of its light decreased as it shifted towards the zenith, the centre of the arch extending in breadth as its length diminished, till its appearance, about 10 h. 50 m. resembled a faintish, nearly circular lake of light, occupying the whole section of the heavens between Ursa Major and the Polar star. At this time, five or six irregular shaped detached patches of bright light were seen to be rapidly evolved. These polygonal patches, for a few minutes, varied in shape and luminosity, and, in a vibrating or pulsating manner, appeared by turns to attract and repel each other, till they slowly amalgamated, forming a segment of a brilliant arc of from 15° to 20° in length, running on a meridian to the west of the first arch, at about half the elevation, with its centre about 35° or 40° from the horizon. The latter arc was of very brief duration, for at eleven o'clock it had entirely passed away. The beauty and splendour of the aurora, though much impaired by the brightness of the moon, then in the same quarter of the heavens, were still rather striking for the latitude of London [where this observation was made], and the arch had the peculiarity of having its direction nearly north and south, instead of at right angles to that course. During the evening several 'shooting stars' were noticed, their course also

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being from north to south. As a usual consequence of these magnetico-electric phenomena, the weather has been very changeable." —*Correspondent in Literary Gazette*, Jan 10, 1846.

Contact Experiment.—"I have succeeded, by the following method of proceeding, in making the electricity of contact unequivocally perceptible without conversation. A strip of silver was fastened to one end of a strip of zinc, the whole was then bent into the form of a V; between the two arms a narrow strip of gold leaf was suspended, so that it could be moved without difficulty to and fro. By carefully moving this gold leaf, it was very easy to place it in such a position that it should be attracted by the piece of zinc only when negatively, and the strip of silver only when positively electrified. As it was electrified by induction, on removing the electrifying bodies (sealing-wax and glass gently excited by friction), the opposite attraction to that produced by the approximation of them took place, if an attraction had been thus produced. In my opinion we have here no ambiguity. There is scarcely occasion to remark, that the apparatus was enclosed in a glass vessel, as the normal attraction ensued only when the gold leaf was in a certain position (*i.e.* exactly midway between the arms of the compound strip), the small intensity of the electricity of contact is perfectly intelligible. Thus if the gold leaf be approximated to another arm, its free electricity will overpower the weak electricity of contact in this arm, and act upon it by induction, whence attraction between the two must always ensue, as the experiment indicates. I have some hopes of being able to obtain a comparative admeasurement of the intensity of the electrical effects of contact by this method."—Henrici.—*Poggendorff's Annalen*, vol. lxiv. Translated into *Phil. Mag.* Sept. 1845.

Light and Magnetism.—"Mr. Hearder stated that he had a communication to make to the Society, which would prove of great local interest, it being connected with a discovery lately announced to the scientific world by Professor Faraday, and which was at present exciting great interest in the metropolis and elsewhere, viz., the Magnetization of Light. Mr. Hearder briefly detailed Professor Faraday's discovery nearly as follows:—A ray of light from an argand lamp is polarized, by reflection, from a plane glass mirror, and is received upon Robert's eye-piece (or slice of tourmaline) through which it is viewed. It is one of the properties of the tourmaline to transmit a ray of polarized light only in certain positions. Professor Faraday, then, had received a ray of polarized light upon a tourmaline placed so as to intercept its passage; under these conditions he brought a powerful electro-magnet to act upon the ray of polarized light, and immediately it became visible through the tourmaline, showing that it had undergone a degree of torsion, the amount of which could be ascertained by noticing the arc of the circle, which it was necessary for the tourmaline to describe before

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again losing sight of the ray. This was, in substance, Dr. Faraday's discovery. Mr. Hearder then detailed an experiment made two years ago by J. Tucker, Esq., of Trenaton Hall:—A ray of light, polarized either by reflection from a mirror or by transmission through Iceland spar, was received upon a tourmaline, as in Professor F.'s experiment; the tourmaline was turned so as to intercept the ray; two disks of sheet copper, standing parallel to and at a small distance from each other, were placed so as to allow the ray of polarized light to pass between them; one of the disks was then caused to rotate by a multiplying wheel, the second disk being unconnected with the first, rotated by magnetic induction; and under these conditions the ray of polarized light immediately reappeared through the tourmaline. Various other experiments, such as the complementing of coloured rays, were also made by that gentleman, who considered that he had thus established some connection between electricity, magnetism, and light. Mr. Hearder stated that he was so much struck with the ingenuity and novelty of the experiments that he strongly advised Mr. Tucker to prosecute them further, and would also have investigated the subject himself but from the circumstance of his deprivation of sight, which, as the Society was aware, would put experiments on Light quite out of his line."—*Dover and Cornwall Natural History Society*, Jan. 5, 1846.

Omnia.—Petrina has found that electricity, when propagated in a rectilinear direction in the atmosphere, in passing by a screen (a metallic conducting plate, connected with the earth), suffers diffraction, just as rays of light and heat do.—In the *Phil. Mag.* of September last, Professor Schoenbein gives fifteen points in which ozone resembles chlorine.—Mr. Joule thinks he has proved "the existence of an equivalent relation between heat and the ordinary forms of mechanical power."—Mr. Hunt has proved that the silver film reduced by light does not conduct the current of a voltaic battery.—M. de la Rue has examined the residue of zinc used for voltaic plates, and discovered perfectly clear and well-formed crystal in right rhombic prisms, which he found to be an alloy of zinc, iron, lead, and copper, in the proportions of 90·0, 2·56, 6·0 and 1·44 per cent.—Mr. Larning does not agree in Faraday's view of "the nature of matter," because he does not see "that absolute continuity is necessary to electrical conduction."—Professor Draper has proved that "the rays of incandescent lines of an oil lamp, and of the sun can excite phosphorescence through glass; and differ from those of an electric spark, or galvanic discharge, in which that peculiarity is deficient."—Professor Wartmann's memoir referred to in p. 146 of the present volume of the *Electrical Magazine*, will be found in the Dec. number of the *Phil. Mag.*—A student's Philosophical Society has been formed in London under the name of "the Faraday Society;" their motto is '*non sumus, sed simus.*"—His Holiness has ordered some books on *Galvanism* to be burnt, taking the word to be synonymous with *Calvinism!!*

* Vide *Elec. Mag.* Vol. I. p. 295.

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ELECTRICAL MAGAZINE.

JULY, 1846.

CLII. *Experiments Relative to Spots on the Sun.* Prof. HENRY. From Proceedings of American Phil. Soc. June 20, 1845.

HIS attention was directed to this subject by an article in the September number of the *Annales de Chimie*, by M. Gautier, upon the influence of the spots on the sun on terrestrial temperature. It is well known that Sir William Herschel entertained the idea that the appearance of solar spots was connected with a more copious emission of heat, and that the seasons during which they were most abundant were most fruitful in vegetable productions; and, pursuing this idea, he was led to trace an analogy between the price of corn and the number of solar spots, during several successive periods. The result of this investigation, so far as it was extended, seemed to favour the views of this distinguished philosopher. A mode of investigation of this kind, however, is not susceptible of any great degree of accuracy; the price of corn is subject to so many other causes of variation besides that of solar temperature, that little reliance can be placed on it.

M. Gautier has attempted to investigate the influence of the solar spots on terrestrial temperature, by comparing the temperature of several places on the earth's surface, during the years in which the spots were most abundant, with those in which the smallest number was perceptible. From all the observations collected, it seems to be indicated, that during the years in which the spots were the greatest in number, the heat has been a trifle less; but the results are far from being sufficiently definite to settle the question: and M. Gautier remarks, that a greater number of years of observation, at a

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greater number of stations, will be necessary to establish a permanent connexion between these phenomena.

The idea occurred to Prof. Henry, that much interesting information relative to the sun might be derived from the application of a thermo-electric apparatus to a picture of the solar disc, produced by a telescope, on a screen, in a dark room. This idea was communicated to Prof. Alexander, who readily joined in the plan for reducing it to practice. It was agreed that they should first attempt to settle the question of the relative heat of the spots as compared with the surrounding luminous portions of the sun's disc. The first experiments were made on the 4th of January, 1845. Mr. Alexander had observed, a few days previous, a very large spot, more than 10,000 miles in diameter, near the middle of the disc. To produce the image of this spot, a telescope of four inches aperture, and four and a half feet focus, was placed in the window of a dark room, with a screen behind it, on which the image of the spot was received. The instrument was placed behind the screen, with the end slightly projecting through a hole made for the purpose, and a small motion of the telescope was sufficient to throw the image of the spot off or on the end of the pile. The spot was very clearly defined, and might have been readily daguerreotyped, had the telescope been furnished with an equatorial movement. The form of the penumbra of the spot, as it appeared on the screen, was that of an irregular oblong, about two inches in one direction and an inch and a half in the other. The dark central spot within the penumbra was nearly square, of about three-fourths of an inch on the side, and a little larger than the end of the thermo-pile.

The method of observation consisted in first placing, for example, a portion of the picture of the luminous surface of the sun in connection with the face of the pile, and after noting the indication of the needle of the galvanometer, the telescope was then slightly moved, so as to place the dark part of the spot directly on the face of the pile, the indication of the needle being again noted. In the next set of experiments the order was reversed; the picture of the spot at the beginning of the experiment was placed in connection with the pile, and afterward a new part of the luminous portion of the disc was made to occupy the same place.

The thermo-electrical apparatus used in these experiments was made by Rumhkorff, of Paris; and in order to render the galvanometer more sensitive, two bar magnets, arranged in the form of the legs of a pair of dividers, were placed with the opening downwards, in a vertical plane, above the needle, so

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that, by increasing or diminishing the angle, the directive power of the needle could be increased or diminished, and, consequently, the sensibility of the instrument could be varied, and the zero point changed at pleasure.

In the present experiments, in order to mark more definitely the difference in temperature, after the needle had been deflected by the heat of the sun, the magnetic bars above mentioned were so arranged, as to repel it back to near the zero point, so that it might, in this position, receive the maximum effect of any variation in the electrical current.

Twelve sets of observations were made on the first day, all of which, except one, gave the same indication, namely, that *the spot emitted less heat than the surrounding parts of the luminous disc.* The following is a copy of the record made at the time of the observations. The degrees are those marked on the card of the galvanometer, and are of course arbitrary.

Spot, $3\frac{1}{4}°$	Sun, $5\frac{1}{4}°$
Sun, $4\frac{1}{2}°$	Spot, $4°$
Sun, $3°$	Spot, $4\frac{1}{2}°$
Spot, $1\frac{3}{4}°$	Sun, $5°$
Spot, $2°$	Sun, $4\frac{1}{2}°$
Sun, $3°$	Spot, $3\frac{3}{4}°$
Sun, $2\frac{1}{2}°$	Sun, $2°$
Spot, $2°$	Spot, $3\frac{1}{4}°$*
Spot, $2°$	Spot, $0\frac{3}{4}°$
Sun, $2\frac{1}{4}°$	Sun, $2\frac{1}{2}°$
Spot, $4\frac{3}{4}°$	Sun, $1\frac{1}{4}°$
Sun, $5°$	Spot, $0°$

The change in the temperature during the intervals of observation, is due to the variations in the temperature of the room differently affecting the two extremities of the pile.

In consequence of cloudy weather, another set of observations were not obtained until the 10th of January, and at this time the spot had very much changed its appearance; the penumbra, while it retained its dimensions in one direction, was much narrowed in the other, and the dark part was separated into two small ones; also the sky was not perfectly clear, and therefore the results were not as satisfactory as those of the previous observations; the indications were, however,

* At this observation a slight cloud probably passed over the sun's disc.

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the same as in the other sets, exhibiting a less degree of heat from the spots.

Cloudy weather prevented other observations on the heat of different parts of the sun, particularly a comparison between the temperature of the centre, and the circumference of the disc, which would have an important bearing on the question of an atmosphere of the sun. The observations will be continued, and any results of interest which may be obtained, will be communicated to the Society.

CLIII. *On a simple method of protecting from Lightning Buildings with Metallic Roofs.* By Prof. Henry. From Proceedings of American Phil. Soc. June 20, 1845.

On the principle of electrical induction, houses thus covered are evidently more liable to be struck than those furnished either with shingle or tile. Fortunately, however, they admit of very simple means of perfect protection. It is evident, from well established principles of electrical action, that if the outside of a house were encased entirely in a coating of metal, the most violent discharge which might fall upon it from the clouds would pass silently to the earth without damaging the house, or endangering the inmates. It is also evident, that if the house be merely covered with a roof of metal, without projecting chimneys, and this roof were put in metallic connection with the ground, the building would be perfectly protected. To make a protection, therefore, of this kind, the Professor advises that the metallic roof be placed in connexion with the ground by means of the tin or copper gutters which serve to lead the water from the roof to the earth. For this purpose, it is sufficient to solder to the lower end of the gutter a riband of sheet copper, two or three inches wide, surrounding it with charcoal, and continuing it out from the house until it terminates in moist ground. The upper ends of these gutters are generally soldered to the roof; but if they are not in metallic contact, the two should be joined by a slip of sheet copper. The only part of the house unprotected by this arrangement will be the chimneys; and in order to secure these, it will only be necessary to erect a short rod against the chimney, soldered at its lower end to the metal of the roof, and extending fifteen or twenty inches above the top of the flue.

Considerable discussion in late years has taken place in reference to the transmission of electricity along a conductor; whether it passes through the whole capacity of the rod, or is principally confined to the surface. From a series of experi-

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ments presented to the American Philosophical Society, by Professor Henry, on this subject, it appears that the electrical discharge passes, or tends to pass, principally at the surface; and as an ordinary sized house is commonly furnished with from two to four perpendicular gutters (generally two in front and two in the rear), the surface of these will be sufficient to conduct, silently, the most violent discharge which may fall from the clouds.

Professor Henry also stated, that he had lately examined a house struck by lightning, which exhibited some effects of an interesting kind. The lightning struck the top of the chimney, passed down the interior of the flue to a point opposite a mass of iron placed on the floor of the garret, where it pierced the chimney; thence it passed explosively, breaking the plaster, into a bedroom below, where it came in contact with a copper bell-wire and passed along this horizontally and silently for about six feet; thence it leaped explosively through the air a distance of about ten feet, through a dormer window, breaking the sash, and scattering the fragments across the street. It was evidently attracted to this point by the upper end of a perpendicular gutter, which was near the window. It passed silently down the gutter, exhibiting scarcely any mark of its passage until it arrived at the termination, about a foot from the ground. Here again an explosion appeared to have taken place, since the windows of the cellar were broken. A bed, in which a man was sleeping at the time, was situated against the wall, immediately under the bell-wire; and although his body was parallel to the wire, and not distant from it more than four feet, he was not only uninjured, but not sensibly affected. The size of the hole in the chimney, and the fact that the lightning passed along the copper wire without melting it, show that the discharge was a small one, and yet the mechanical effects, in breaking the plaster, and projecting the window frame across the street, were astonishingly great.

These effects the Professor attributes to a sudden repulsive energy, or expansive force, developed in the air along the path of the discharge. Indeed, he conceives that most of the mechanical effects which are often witnessed in cases of buildings struck by lightning may be referred to the same cause. In the case of a house struck within a few miles of Princeton, the discharge entered the chimney, burst open the flue, and passed along the *cockloft* to the other end of the house; and such was the explosive force in this confined space, that nearly the whole of the roof was blown off. This effect was, in all probability, due to the same cause which suddenly expands the air in the experiment with Kinnersly's electrical air thermometer.

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Note.—Dr. Patterson stated, that Mr. Jefferson was of the opinion that metal roofs protected buildings, not from being struck, but from the danger of the stroke; the contrary opinion is generally held, but Prof. Henry's experiments show that Mr. Jefferson was correct. Dr. P. saw the lightning strike a row of dormitories with metal roofs, at the University of Virginia: the flash was very severe, but produced no evil effect; the lightning had spread itself over the surface, and left its mark at each interruption of the conductor, but did no damage. It was said at the University, that the rotunda had been frequently struck without injury.

CLIV. *Account of a Thunder-Storm.* By Rev. W. B. Clarke, M.A., F.G.S. — From the "Sydney Herald." Communicated by the Author.

(*To the Editors of the Sydney Morning Herald.*)

Gentlemen,—The attention of your readers having been invited by your correspondents to the remarkable thunder-storm which visited the neighbourhood of Paramatta, as well as other parts of the country, on Thursday, 20th November, I beg to forward to you the notes I made at the time, believing that they may be not uninstructive to persons interested in the history of such tempests.

The preceding day had a mean temperature of 71° 25′ with a range of 22°, and a mean pressure of 30·0025 inches; the wind till about 5·30 p.m. being from north and north-east; after which it was replaced below by a south-west current, the cirrous streamers of which were broken up by contact with the upper north-east air, which still floated above it. Notwithstanding this south-west current, the barometer was lower at 9 p.m. than at 2·30, when the north-east sea-breeze blew at the surface. The conditions of the atmosphere before and after the change of wind may be collected from the following tables:—

Wednesday, 19th November.

Hour, 2·30 p.m.; bar. 30·00; t. a. 72; t. e. 77; wet, 65; dew point, 57·68; dryness, 19·32; evaporation, 2·20 grains; vapour plane, 5703 feet.

Hour, sunset; bar. 20.93; t. a. 70; t. e. 68; wet, 61·50; dew point, 57·23; dryness, 10·77; evaporation, 0·64 grains; vapour plane, 3024 feet.—Moon in apogee at 7 p.m.

Thursday, 20th November.

Hour, 6·30 p.m.; bar. 30 04; t. a. 70; t. e. 67; wind, N. E.—

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Stratus and cirri in N. W. Light air, and a few drops of rain at 8 a. m.

First Storm.—First Phase.

Hour, 8·30 p. m.; bar. 30·05; t. a. 69; t. e. 65; wind, N. E. —Heavy and dull, W. and N. W.

Hour, 9·30 p. m.; bar. 30·04; t. a. 69·5; t. e. 68; wind, N. E. nearly calm. Haze and stratus over the sky, black in S. W.

Hour, 10·0 p m.; bar. 29·93; t. a. 69·5; t. e. 70; wet, 65; dew point, 62·10; dryness, 7·90; evaporation, 0·81 grains; vapour plane, 2280 feet; wind, N.—Thunder in N. W. and W., clouds rapidly forming and passing to S. Northern quadrant clear.

Hour, 10·30 p. m.; bar. 29·93; t. a. 69·5; t. e. 71; wet, 64; dew point, 60·33; dryness, 10·67; evaporation, 1·10 grains; vapour plane, 3351 feet.—Storm collecting in S.; the clouds passing thither from all points; chiefly stratus in W.; N. and N. E. This storm is to S. W. of Paramatta, but is going from S. W. to S. E. by way of N. and E. of its locality.

Hour, 10·35 to 10·45 p. m.; winds, N. and W.—Dense stratus in S. and S. W.; white *bicuspidated* lightning from the edges, followed by thunder, at 10 seconds, 45 degrees high in W. by S.

Scud rising in S.; clouds in zenith from N. to S., and in southern quadrant from W. to E.

Hour, 10·45 to 10·50 p. m.; bar. 29·93; t. a. 70; t. e. 71; wet, 63; dew point, 57·94; dryness, 13·06; evaporation, 1·32 grains; vapour plane, 3384 feet; wind, N., N. W., W. and S., S. E.—Few drops of rain. Clouds in rapid passage from points indicated, to the storm. *Surface wind, S. E.*

Hour, 10·58 p. m.—Clouds drawing from N. E. to S. W. Scud *above* seen through, passing from S. W., N., N. E., and S. These clouds thus gyrate from N. E. to S. E. by way of S., the storm going the contrary way.

Hour, 11·0 p. m.—Fresh puffs of S. E. wind in surface. Cumuli seen behind the scud in W. by N.

Hour, 11·12 p. m.; bar. 29·93; t. a. 69·5; t. e. 70; wind, S. E. —Upper clouds passing from N. to S. and from S. to E., dull grey stratus in S. E.

Second Phase of First Storm.

Hour, 11·30 p. m.; bar. 29·91; t. a. 70; t. e. 70; wet, 63; dew point, 59·12; dryness, 10·88; evaporation, 1·18 grain; vapour plane, 3354 feet; wind, S. E.—Thunder in S. 45 degrees high, few drops of rain. Three currents in north quadrant, upper and lower from N. E, middle from S. W.

Hour, 11·35 p. m.; bar. 29·90; t. a. 70; t. e. 71; wet, 64; dew point, 60·33; dryness, 10·67; evaporation, 1·10 grains; vapour plane, 3351 feet; wind, N. E.—*Conditions same as at* 10 *hours* 30 *minutes* a. m. Air warm. Surface wind light N. E. Clouds in S. E. heavy and dull grey. Lightning vertical, and thunder twenty-one seconds after.

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Hour, 0·30 p. m.; bar. 29 89; t. a. 70; t. e. 76; wet, 66; dew point, 60·16; dryness, 15·84; evaporation, 1·805 grains; vapour plane, 4620 feet; winds, N. W., N. and E.—Three currents above, highest from N. Surface wind, E.

Hour, 1·0 p. m.; bar. 29·87; t. a. 74; t. e. 77; in sun, 100; wind, N. W., N. E., E.—Clouds gathering in N. W. Surface wind, E. Upper current, N. W.

Hour, 1·45 p. m.; bar. 29·86; t. a. 73; t. e. 77; in sun, 90; wet, 67; dew point, 61·36; dryness, 15·64; evaporation, 1·86 grains; vapour plane, 5476 feet; wind, N. W. and E.—Clouds moving slowly from N. W. and N. E.

Hour, 2·0 p. m.; bar. 29·86; t. a. 74; t. e. 78; in sun, 130; wet, 67; dew point, 59·70; dryness, 18·00; evaporation, 2·21 grains; vapour plane, 4983 feet; Wind, W., E.—Light sea breeze from E.

Hour, 2·30 p. m.; bar. 29·85; t. a. 76; t. e. 78; in sun, 112; wind, W., E.—Other conditions as at 2 p. m. Clouds above from west in blue sky. Light sea breeze from east. Heavy clouds gathering in S. W. all the afternoon.

Second Thunder-storm.—First Phase.

Hour, 4·50 p. m.; bar. 29·80; t. a. 72; t. e. 74; wind, S. E. —Sky overcast with dense stratus and cirri streaming from N. W. Thunder and lightning in N. W. *Surface Wind, S. E.* Upper current from W. N. W.

Hour, 5·45 p. m.; bar. 29·80; t. a. 72; t. e. 74; wet, 65; dew point, 59·58; dryness, 14·42; evaporation, 1·58 grains; vapour plane, 4251 feet; wind, S. E.—Few drops of rain. Storm in north quadrant.

Hour, 5·55 p. m.; bar. 29·80; t. a. 72·5; t. e. 74; wet, 65.—White lightning. (Thunder in 21″ from N. and N. W.) Storm passing to E. and S. E. A few large drops of rain.

Hour, 6·0 p m.; wind, E. to N. E.—Same conditions continued since 5 hours 45 minutes. Sky hid by jagged undefined cirri and stratus, grouping in patches which stratify from S. W., but are passing from N. W. Thunder distant. Light airs occasionally from E. and N. E. Smoky patches of cirri flying off to southward from the edge of advancing storm. Light rain at temperature of seventy-two degrees. We are on N. E. edge of storm at present.

Hour, 6·20 to sunset 6·57; bar. 29·80; t. a. 72; t. e. 71 to 70; wet, 65; dew point, 62·52; dryness, 8·48; evaporation, 0·91 grains; vapour plane, 2799 feet; wind, E.—Rain over. Lightning in W. S. W. Thunder in 5″ wind fresh from east. Heavy storm coming. Our zenith will be on N. E. of its axis. *Tricuspidated* white and red lightning in S. W., four or five flashes at a time, white and horizontal from the edge of the clouds, thunder in N. W. and S. W. Sometimes the lightning runs along the surface and lightens all the edges of the clouds, dashing about in extraordinary

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curves and zigzag lines, followed by heavy rolling and crashing peals of thunder, at intervals of from five to twenty-one seconds. At 6h. 35m. the lightning was very fearful to look at, and gave the idea of darts of fire thrown out on all sides as represented in the thunderbolt of the classical Jove. One flash at sunset was like nothing so much as the tortuous curves of a long stock-whip suddenly cracked overhead. This effect was due to the lightning following the sinuosities of the edges of the clouds. The whole storm for about half an hour was perfectly terrible.

Hour, 7·15 p. m.; bar. 29·84; t. a. 71; t. e. 69; wet, 65; dew point, 62·73; dryness, 6·27; evaporation, 0·704 grains; vapour plane, 1866 feet; wind, S. E.—Hail and heavy rain, preceded by a most alarming noise, quite different from that of wind or thunder. In fact it was calm nearly the whole time. The intervals between the lightning and thunder were frequently two and three and even only *one second*. Rain came from S. W. The edges of the clouds illuminated by incessant flashes, which occasionally spread over the whole *under surface* of the clouds, where the chief tempest was; but so soon as the earth became charged by the rain and hail, vertical white lightning, with red edges, and having *a secondary* or *reflected flash behind* (probably an optical illusion), descended from the clouds, and appeared to rest for half a second or more in one position, followed instantaneously by a fierce explosive thunder-clap like the bursting of heavy ordnance.

Hour, 7·30 p. m.; bar. 29·84; t. a. 71; t. e. 66; wet, 63; dew point, 61·11; dryness, 4·89; evaporation, 0·47 grains; vapour plane, 1371 feet; wind, N. and S. W.—Rain light and heavy alternately, sometimes poured out in torrents after the lightning flash; heavy hail at times preceded by noise; wind in puffs; low scud from N., seen by glare of lightning; ascending stroke observed in S. E.; thunder re-echoed in S. and S. E. continually.

Hour, 8·0 p. m.; bar. 29·84; t. a. 71; t. e. 67; wet, 64; dew point, 62·21; dryness, 4·79; evaporation, 0·46 grains; vapour plane, 1359 feet; wind, E. to S.—Storm in N. and N. E.; a few cirrous clouds spitting out lightning from S. W.; the N. and N. W. quadrant incessantly illumined by sheet lightning from the *upper* surfaces of the clouds, with an occasional vertical up or down stroke; thunder distant; *surface wind in gentle puffs from E. to S. E.*; rain fallen since 7h. 15m. *eight-tenths of an inch*; stars out; calm.

Second Storm.—Second Phase.

Hour, 8·30 to 8·35 p. m.; bar. 29·835; t. a. 71; t. e. 67; wind, S. E.—The other conditions as at 8 o'clock. Incessant lightning from low cirro-stratus in N. by W. Remaining clouds drifting slowly to N. E. At 8h. 35m. lightning in W. and N. W.; fleecy cirri in clear sky; Venus shining brightly: light rain in zenith; incessant thunder in N. W.; calm.

Hour, 9·0 p. m.; bar. 29·82: t. a. 70; t. e. 67; wet, 63; dew

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point, 60·8; dryness, 6·12; evaporation, 0·60 grains; vapour plane, 1902 feet; wind, S. E.

Hour, 11 0 p. m.; bar. 29·83; t. a. 71; t. e. 66; wet, 63; dew point, 61·21; dryness, 1·79; evaporation, 0·17 grains; vapour plane, 1371 feet; wind, S. E.—Thunder heavy, at the height about 10·45, in W. and N. W., with lightning for a time during the night.

FRIDAY, 21ST NOVEMBER.

Hour, 5·30 a. m.; bar. 29·78; t. a. 70; t. e. 66; wet, 63; dew point, 61·11; dryness, 4·89; evaporation, 0·47 grains; vapour plane, 1371 feet; wind, N. W.—Calm, hazy, and partly overcast by stratus.

The conditions of the atmosphere at 5·30 a. m., 21st November being, as may be seen by comparison, the same as at 7·30 of the 20th.

From the above tables, we learn that the first storm was about six hours in progress, each phase occupying about half the time; and the height of each phase in that storm occurred about two hours and a quarter from the commencement. In the second storm, which lasted somewhat longer, the phases were nearly equal in duration, and the height of each phase also intermediate; and this I have observed in very many thunder-storms in this colony, which appear to obey invariable laws, however they may seem to the ordinary observer to differ from each other. The centres of these storms do not, of course, always pass over the same spot, but whenever I have observed the centre in the zenith, or the effects of the storms from their edges, I have constantly seen similar results under each aspect. The course of these storms appears to be from S.W. to S.E., gyrating by way of W. and N.; there are always two currents (if not more), one of which is from S.E., and another from N.W.; the surface wind blowing to the advancing storm till it reaches an altitude of about 45°. Then there is an agitation of the clouds, which are perfectly still in the zenith, at the time when the centre passes that point, after which the upper wind veers and follows the storm. But always the north-east wind, or its varieties from east to south-east, occupies the surface, continuing from south-east sometimes for a day or two afterwards, in consequence, probably, of the effect of the storm to north-west. I think my observations enable me to say correctly, that if in this part of the country a tempest appears first in south-west or south, it will pass over the locality; if it appears first in north-west, unless a very extensive one, it will pass to the southward, without crossing the zenith; a law which results from the fact that the gyrations of the storm follow the constant course indicated above. But the winds

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do not veer in the same order as the storm; that being due to currents of different density, moisture, and temperature, the electricity of which is developed at their planes of contact; and as the lower currents ascend, their upper gyration must be due to the motion of the upper current, which, theoretically as well as from observation, produces an opposite gyration.

In the late storm the hail was not severe at Paramatta, but along the range of Seven Hills, Baulkham Hills, &c., which are a few hundred feet nearer the clouds, great damage was done by enormous *ice balls*, which destroyed the orchards, and *killed poultry and pigs*. Some of this ice was still unmelted on the 26th, *five* days afterwards, though the temperature at times, in the sunbeam, has been as high as 140°: an idea may therefore be formed of the compactness of this aërial grape shot, *some of which have been described to me* as measuring nearly *seven inches in circumference*.

The hail fell at the South Head (see Report) between one and two P.M. This was at the close of the first thunder-storm here. By comparison of notes in my possession, from different places, I find that this first storm travelled about fifteen miles per hour on the line of its axis. The second storm, which was more extensive in its range, travelled about twenty miles per hour. On laying down the various data it will be seen that each traversed in the same general course, and gyrated from *left* to *right;* but that the axis of the first storm was more to the south than the second.

The rain in the latter did not amount to an *inch* here; but it must have been much more abundant along the line of hills, as the dam was overflowed and a considerable fresh in the river during the night and early morning. This abundance of rain must have fallen in the neighbourhood of Prospect and Seven Hills, because they are the sources of drainage. It has been conjectured with reason that in many cases subterraneous waters issue during such thunder-storms as these.

Paramatta, *November* 28. W. B. C.

CLV. *On the Condensation of Magnetic Force upon the Surface of Magnets.** By M. de HALDAT. (Extract from the Memoirs of "The Royal Society of Sciences, Letters, and Arts, at Nancy.) Archives de l'Electricité. Vol. v. p. 394.

(TRANSLATION.)

WHEN the result of the researches made upon the concentration of the magnetic force upon the surface of magnets

* The abstract of this paper is given in *Electrical Magazine*, Vol. I. p. 597.

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was presented to the Academy of Sciences, several members of that illustrious body appeared to desire that this law should be more vigorously determined; and that we should assure ourselves whether this force is accumulated at the surface of bodies in a magnetic state, as the electric force is at the surface of electrized bodies, or whether it is simply accumulated *near* the surface of the magnets, according to the expression that I had employed to characterize the fact. Not being able to satisfy the desire of the learned academicians, except by means of new researches, I have undertaken them, and included them in this supplement to the first memoir.*

The mode of investigation was not merely indicated, but it was even obligatory, if I were not desirous of being deprived of the term of comparison between the old and the new experiments. In order to prove the concentration of the magnetic force toward the surface of magnets, I took a gun-barrel, *a*, one portion forming a tube 10 c.m. (4 in.) long, and 22 m.m. (·8 in.) exterior diameter, the weight of which was 280 grammes (4322 grs.); I introduced into it a cylinder of soft iron, which filled its capacity; it was 16 m.m. (·62 in.) less in length, and weighed 340 grammes (5248 grs.). The iron tube was contained in a brass jacket enveloped in copper wires covered with silk, to produce in it the magnetic condition. I proved the concentration toward the surface of the tube, by comparing the magnetic force, which it possessed, when it contained the additional cylinder, with that which it preserved when it was separated from it.

Wishing to demonstrate that the magnetic force is concentrated very near to the surface, since it is impossible to prove that it is absolutely at the surface; and, as this could only be done by preserving the surface of the same magnet, and gradually diminishing its mass, I took in the same gun-barrel, two other portions, perfectly equal in length and capacity; I diminished the mass, on the latter, so that the second portion, *b*, weighed only 160 grammes (2270 grs.), and the third, *c*, only 90. Moreover, the whole three were armed with the same jacket, and filled or deprived of the solid cylinders, whose weight, as I have mentioned, was constantly 340 grammes.

The differences in the magnetic force communicated in the three pieces of our apparatus were appreciated, as in the former memoir, by the sine-compass, but principally by the oscillations of the needle, which presents results that are more constant, and are more easy to be proved. The needle em-

* *Electrical Magazine*, Vol. I. p. 353.

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ployed was magnetised to saturation, was very mobile, and was fixed in an invariable position. It indicated and measured the relative forces of the three magnets by the number of oscillations executed in a period of 45″. The apparatus remained constantly to the direction of the magnetic equator; and we, 1st, estimated the force communicated to the needle by the terrestrial current; 2nd, the same force, resulting from the action of the earth and the proper force of the pieces of the apparatus, acting by their own energy; 3rd, of this same force, resulting from induction; and, finally, 4th, of all these forces united and combined, as indicated in the following table:—

	Influence of the earth on the full tube.	Influence of the earth on the empty tube.	Influence of the induced current on the full tube.	Influence of the induced current on the empty tube.
	Grammes.	Osc.	Osc.	Osc.	Osc.
A weighs	200	15 to 16	14 to 15	72	72
B „	160	15 „ 16	14 „ 15	72	71
C „	90	15 „ 16	15	71	70

A needle of less dimension than the former, but giving a greater number of oscillations in the same time, was consulted comparatively; and although, on account of their rapidity, some error may have entered into the computation of these oscillations, they yet confirmed the experiments that we have just related, not having varied except between 89 and 91 oscillations, always in 45″, whether the tube were provided with the additional cylinder or were deprived of it.

The experiments made with the sine-compass, as in the former memoir, have also concurred to confirm the accuracy of the results that we have just related, whether each of the tubes were magnetised by induction, or were provided with, or deprived of, the additional cylinder. If we now compare together the results of the experiments executed with the needle, we shall find that, on deducting 40 oscillations produced by induction, and 22 by the action of the earth, there remain 18 or 20 representing the magnetic state of the empty and full tubes; and as, with very different masses, their surface differs very little, the striking equality of the results may be attributed to the surface on which the magnetic charge is accumulated.

Having reduced the tube *c*, No. 3, to a thickness which did not exceed that of the sheet iron employed to conduct the

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smoke of stoves, and being compelled to renounce any greater degree of thinness, I felt it necessary to seek for other proofs of the concentration of magnetic force upon the surface of magnets. I thought to have found them in the procession of magnetism by rotation, by submitting metal discs of the feeblest degree of thickness to the influence of a rotating magnet. Indeed, the rotation in this class of experiments, according to the theory that we have adopted,* being produced only by the instantaneous development of the magnetic force in the parts of the disc corresponding to the poles of the magnet, these facts are evidently connected with the subject on which we are treating. As the discs employed are entirely superficial, we are not necessarily led to consider the effects as arguments in favour of the concentration of the magnetic force on the periphery of the bodies in which it is developed? Whatever further explanation may be offered, these facts will always present curious phenomena, which invincibly prove the little influence of mass in the development of magnetism.

The conditions required for the success of these experiments consist, 1st, by giving the discs a diameter equal to the length of the rotating magnet; 2nd, by diminishing as much as possible their force of inertia by the diminution of their mass, which is obtained by gluing the metal leaves upon pasteboard, when they are too thin to preserve their form, while placed horizontally; 3rd, by suspending the discs to double films of the cocoon with little hooks 5 or 6 cent. long, fixed to the summits of little wooden cones, the bases of which are fixed to the centre of the upper surface of the disc with an adhesive substance; 4th, by maintaining them in a horizontal situation, and at the smallest possible distance from the magnet, from which they should be separated only by the thin glass that covers the rotating magnet.

The discs that I have employed are of very different kinds: our present concern is only with those that are formed of metal substances. Being more dense than other substances, the metal uniting under a given volume a greater number of molecules, they are necessarily the more easily magnetisable; and the application of the magnetic agent to bodies of this class is the more efficacious in producing their rotation, inasmuch as several being susceptible of being reduced to thin films are the more advantageously subjected to the action of the rotating magnet, and experience its influence more easily. However, the differences which they present do not correspond with their density, so that iron and copper, whose

* Annales de Chim. et de Phys. Vol. lxvii. p. 203.

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density is less than that of several amongst them—as platinum, gold, and lead—are, however, much more easily magnetisable.

I had endeavoured to appreciate approximately these differences by a microscopic capstan, on which the suspending wire of the disc was wound. This little instrument is fixed at the summit of the bell-glass by which the discs are sheltered, and through the neck of which the suspending film is transmitted to them. The metal discs, whose thickness is too inconsiderable to preserve their form in a horizontal position, must be fixed, as I said, on discs of pasteboard. It would be too bold to affirm that hardness is absolutely without influence; but here, as the surface is in other respects great in respect to the solidity, these discs furnish a still more favourable argument to the opinion put forth in the first part of this complementary memoir.

The manner of determining the rotation of the metal discs differs in nothing from the ordinary process. A uniform rotation and a mean velocity is what suits best; only, when these discs appear but little disposed to receive the influence of the magnet, the rotation may be determined by commencing by the method of oscillations; and even by impressing upon the discs the slightest possible movement, which is accelerated, as I have elsewhere explained,* by making the magnet oscillate. By aid of this manœuvre, they finally acquire a movement, which is indeed very slow, but which is rendered more manifest by placing a mark on their upper surface.

Ductility being the indispensable condition for reducing metals into thin films, it would seem that for those which are deprived of it, we should be deprived of the means by which we have recognised the condensation of magnetism on the surface of many metallic substances: but iron, after having lost its cohesion, having shown itself susceptible of acquiring magnetism, and even of forming magnets endowed with polarity, when the particles are collected into long tubes, and are moderately rammed, I presumed that fragile metals, when reduced into powder, would still show themselves sensible to magnetic influence;† and this was, indeed, confirmed by experience. Discs of pasteboard, when suspended as we have described, and covered on their lower surface with the powders of bismuth, antimony, cobalt, and arsenic, fixed with glue, all obeyed the influence of the rotating magnet. We should, however, remark, that we must not expect to see them immediately acquire a rapid movement, as occurs in the discs of iron or copper. This movement is, indeed, neither sudden

* *Mémoires de l'Acad. de Nancy*, 1840.

† *Annales de Chim. et de Phys.* Vol. lxv. p. 214, 1837.

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nor rapid, because it is first necessary that the force of inertia of the disc be surmounted by the action of the rotating magnet; and it is only by the repetition of numerous magnetic efforts, namely, after several revolutions, that this resistance is overcome; and, moreover, because the obstacle produced by the stiffness of the suspending thread must be destroyed. This resistance should not be neglected, because the weight of the discs frequently prevents our employing single threads of the cocoon, but threads composed of at least four of these elementary threads. Their resistance, indeed, becomes evident as soon as the disc is made to execute a semi-rotation; for it then advances only very slowly, it often stops, and returns of itself opposite to the direction that had been communicated to it. These experiments are very simple; however, in order to obtain the desired effect, they require some practice, and, above all, a great amount of patience.

The last portion of my experiments must be considered rather fitted to complete the memoir on the causes of the alteration of the magnetic force,* than to prove the concentration of this force upon the surface of magnets. These experiments were executed with discs of pasteboard covered with metal powders, in which the disposition to receive magnetic influence was weakened or almost extinguished by combination with substances little or not at all magnetizable by the motion communicated—such as sulphur, phosphorus, carbon, oxygen. Compounds of iron and these substances, in fact, show themselves among the most indifferent to the influence of the rotating magnet. The sulphuret of iron is the first on which I observed certain indications of a disposition to receive the magnetic state. The specimen employed belonged to a very regular cube of the natural sulphuret. Another trial was made with the artificial sulphuret, composed of equal parts of both substances; and as this compound, which may be considered as one of the richest in sulphur, showed only feeble dispositions to acquire the magnetic state, it was not unimportant to determine whether these dispositions varied with the proportions of the elements of this compound.

The trials made with a view to this, proved to me that the disposition for magnetism diminished in proportion as the quantity of sulphur increased. Mineralogists had already recognised this truth, by admitting among minerals, which are the least rich in sulphur, a species to which they have given the name of magnetic pyrites. I had hoped to have found in the little microscopic capstan the means of appreciating, at least approximately, the relation that undoubtedly

* *Mémoires de l'Acad. de Nancy*, 1843.

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exists between the disposition for acquiring the magnetic state and the relative proportions of sulphur united to the iron; but the inappreciable distances at which the rotating magnet acts or ceases to act upon the discs have opposed an insurmountable obstacle to my views. The combination of carbon with iron was still less characteristic, on account of the small number of combinations in definite proportions existing between these two substances. The richest in carbon, the tri-carburet obtained by the decomposition of Prussian blue by heat, showed no disposition to acquire the magnetic state, whilst steel and cast-iron are, as we know, easily magnetizable —less so, however, than iron, as we have proved.*

Chemistry, which admits but two degrees of oxidation in iron, leaves us also but little hope of arriving at the solution of the question that we have proposed. Indeed, all the oxides which are regarded as less charged with oxygen than the sesqui-oxide or colcothur, as the saffron of Mars, the oxide obtaining by the decomposition of water, rust, *les batitures* [Fr.], &c., which are considered as oxides of the lowest degree, are all about equally magnetizable, whilst the sesqui-oxide presents an indifference that can only be overcome by Coulomb's process.

These examples are undoubtedly but little fitted for throwing light upon the question of the disposition for magnetism preserved by iron in its combinations. Meanwhile, we cannot but notice that combination does not destroy this disposition, except when the proportion of the body combined with the iron is considerable, as in the sesqui-oxide, which in 100 parts of iron contains nearly half oxygen. This metal, the most magnetizable of all substances of this class, does not therefore lose the property by which it is distinguished, except when the equivalent that is given to it predominates in the combination. Here the magnetic agent shows itself less powerful than light, which experiences the same modifications by compounds as by their elements, as M. Biot proved in his remarks upon the polarization of liquids. All the facts that we have just related have been still further confirmed by another means, which consists in magnetizing tubes of glass filled with the metal powders of which we have spoken, and examining them with an astatic needle of great sensibility.

If, according to the inductions that appear to result from these experiments, we endeavour to descend to the cause of the alteration of magnetic force in combinations of iron, and we seek for it in the laws of affinity, we shall not succeed in

* *Mémoires de l'Acad. de Nancy*, 1838.

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dissipating the obscurity; for there are compounds, in which all or almost all the properties of the elements of the compound are destroyed, whilst there are others in which one or more of these properties partially remain after the combination. We should not be more fortunate in seeking for this cause in the molecular arrangement, since the magnetic condition remains in magnets after they have undergone evident modifications in this arrangement. But could we not suppose what the result would be of the opposition of the magnetic force of the component particles, when, some being positive and others negative, they neutralized each other reciprocally? as I showed in the magnets that are formed of particles of iron without cohesion, whose respective situations are changed by communicating to them shocks capable of making them turn on themselves, and correspond by poles of different names, and by this means neutralizing each other.* Furthermore, I satisfied myself that the mere mixture is not sufficient to modify the disposition to acquire magnetic force. To affinity, therefore, we must trace the most energetic power for diminishing this force in bodies. Were we to devote ourselves to the researches pointed out by these considerations, we might have to examine not only the alteration of the disposition to acquire magnetic force in salts with an iron base, but first of all the modifications produced by alloys of this metal in which this disposition would necessarily seem to be more completely preserved. We already know that, in some of them, this disposition is more or less altered—of which I satisfied myself in respect to the nickel in *German silver;* but these researches require a new work.

From the facts comprised in this second memoir, we may conclude, as I have already done at the end of the first memoir:—

1st. That as the mass exercises very little influence over the power of magnets, the latter necessarily resides in the molecular strata which compose the surface.

2nd. That the magnetic instrument—the phenomena of which are rendered sensible by movement—being able to render this force appreciable in metallic substances, the surfaces of which have an extremely large extent, in comparison with the solidity, an argument may be drawn which is very favourable to the law of the concentration of magnetic force on the surface of magnets.

3rd. That, by means of this instrument, the disposition for acquiring magnetic force may be rendered sensible in metallic

* *Annales de Chim. et de Phys.* vol. lxv. p. 214.

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substances deprived of the force of cohesion between their constituent particles; and even in those which are composed of heterogeneous parts are thence to draw proofs in favour of the universality of magnetism.

CLVI. *The Action of Magnetism on Calorific Radiations.* By Mr. WARTMANN, Professor of Physics at Lausanne. "L'Institut," May 6, 1846.

(TRANSLATION.)

As a source of heat, those contrivances that accompany M. Melloni's apparatus may be used. The greater portion of M. Melloni's experiments were made with Locatelli's lamp, or with an Argand lamp provided with a parabolic reflector. As polarising apparatus, Mr. Wartmann used mica piles. These piles are mounted upon an apparatus, analogous to M. Biot's apparatus for the study of polarized light. Each of them is then fixed upon a frame moveable at the extremity of a hollow cylinder, so that their planes may be brought to make any angle either with each other or with the axis of the tube: for each pair of piles, it is necessary to determine the angle of their surface with the incident rays that give the maximum polarization. A diathermal body,—gem-salt, is placed in the cylindrical tube between the two piles of mica, the planes of which are arranged at right angles. As electrical apparatus, Mr. Wartmann uses a Bunsen's pile formed of fifty-two elements, and a Grove's battery of thirty pairs. The current was passed either in helices of thick wire, the centre of which was occupied by the part of the tube in which the diathermal body was placed, or in different electro-magnets, upon the poles of which the same tube was fixed, and these were made to act separately or simultaneously. The only thermometric apparatus that is sufficiently sensible, is the thermo-electric pile. According to circumstances, two different rheometers were applied: in one of them the system of needles makes but one oscillation in 28″. The rheometer is placed beyond the sphere of attraction of the electro-magnetic instruments: those employed by Mr. Wartmann required a distance of fifteen metres. The upper polar wires coming from the thermo-electric pile are arranged perpendicularly to the spheres of the inducing circuits, so as to diminish as much as possible the induced current, which almost always affects the needle. The final result of each experiment is deduced from the fixed deviation of the index.

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Every thing being arranged as we have just mentioned, the thermo-electric pile is marked, so as to prevent every extraneous radiation, and a pencil of heat is allowed to fall upon it. This pencil is polarized by the mica pile fixed upon the anterior part of the tube: in this condition, it traverses the diathermal substance (having been deprived by two diaphragms of all the rays that would tend to pass on the outside of this substance); it then passes through the second pile of mica, when it is partly extinguished, and finally arrives and acts upon the pile, and produces a certain deviation in the rheometer. When the latter has become fixed, the electro-magnetic influence is made to act. The needle immediately moves, and stops in another position. If the voltaic circuit is then broken, it returns to its point of departure. Thus the plane of polarization of caloric radiation has been turned. We may satisfy ourselves of this, by making the power that sustains the anterior or the posterior pile of mica to traverse a suitable arc. The greatest deviation that Mr. Wartmann has hitherto obtained is from 4° to 5°; it is generally less. This result is not to be wondered at. The values of the rotation found by MM. Faraday, Pouillet, Böttger, &c. for rays of light completely polarized, were always very feeble, when observed by apparatus of extreme sensibility. Now, says Mr. Wartmann, there does not exist, so far as I am aware, any means of polarization applicable to my experiments, which does not allow a small portion of the heat to remain unpolarized. Moreover, the process for measuring calorific rays, through the system that I have employed, are less precise than the phenomena of polarization, without speaking of the influence, which is always deceptive, exercised by the thirty metres of wire employed to connect the rheometer with the thermo-electric pile, and the small degree of the absolute intensity of my voltaic piles.—[A.]

CLVII. *On the Calorific Power of the Light of the Moon.* Extract of a Letter from M. Melloni to M. Arago. "Comptes Rendus," March 23, 1846. Vol. xxii. p. 541.

(Translation.)

A lens, one metre in diameter, constructed by M. Henri Lepante, and intended for the meteorological observatory of Vesuvius, has just reached me. To study without danger the adjustment of the different rings, as well as the distance and extent of the focus, I exposed this magnificent optical instrument to a fine clear moonlight; and I brought the lens, by

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the double rotatory movement of which it is susceptible, into a plane exactly perpendicular to the direction of the rays. The light that fell on the surface of the lens was concentrated at the distance of about one metre, on a circular space one centimetre in diameter. This small circle, which was very brilliant and sufficiently clearly defined, and having a size sensibly equal to the section of the tubes that are fitted upon my thermoscopic piles, suggested to me the idea of trying its action upon these piles. The preparations for accomplishing this experiment were immediately made, and a considerable deviation was developed in the multiplying rheometer as soon as the rays that penetrated into the interior of the tube impinged upon the anterior face of the apparatus. Astonished at the vivacity of the action, and very much doubting as to its being derived from the lunar heat, I placed my hand at a certain distance before the opening, and the index of the rheometer immediately returned to zero, passed beyond, and took a contrary direction—an evident proof that its primitive movement was derived from a *frigorific* radiation, namely, a reduction of temperature in the face of the pile exposed to the focus. The origin of this cold was easily assigned. As the lens was placed in an open balcony, and under a perfectly clear sky, it must necessarily, on account of the great emissive power of glass, radiate its heat abundantly into space, and thus lower its temperature below that of the pile which was enveloped in a metal jacket, and placed in the interior of the apartment. So long as the pile was protected by the metal screen, the feeble radiation of the latter did not permit of it feeling the influence of this cold of the lens; but immediately that the cover was lowered, the calorific exchange took place between the two bodies; and the pile, losing more than it received, must necessarily lower the temperature of its exposed face, and thus produce the electric current that caused the deviation of the needle of the rheometer. To remedy this inconvenience, I removed the lens within the window that opened upon the balcony; I applied a mat to the window, which I could easily raise so as to allow the rays of the moon to enter into the apartment, or let down so as to intercept them. I held the mat down until the equilibrium of temperature was established; and after having convinced myself that no deviation was obtained in the rheometer when the cover of the pile was down—the pile being always in the focus of the lens,—I let the light of the moon fall upon this instrument: a deviation of a few degrees on the side of heat was manifested. I immediately repeated my experiment, and, to my great surprise, the deviation occurred in a contrary direction!

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A few moments' reflection sufficed to convince me that these changes of direction were, in all probability, due to puffs of the external air that entered from time to time into the apartment, and found their way to the exposed face of the thermoscopic body. Things might easily have been arranged, so that the air might not have found access behind the lens; but, guided by the theory of the identity (of heat and light), and the well-known experiment of De Saussure relative to the thermometer placed at the bottom of a glazed case, I thought I should better succeed in accomplishing the object I had in view by introducing into the interior of the tube two glass diaphragms, perfectly diaphanous and well polished on their four sides—the first at a little distance from the pile, and the second very near to the opening. I therefore mounted the tubes of my pile in this manner, and on the first favourable opportunity I repeated the experiment. The index of the instrument first remained stationary for a few moments; it then commenced to deviate slowly; and after four or five minutes, it stopped in a stable manner at an arc of 3°.7. I then moved the pile from the focus, and placed it by the side, its opening still turned toward the centre of the lens; the deviation immediately commenced to diminish, and in a few minutes the index returned to zero. I several times repeated the same operation, removing the pile sometimes on the other side; and the needle always deviated when in the focus, and fell back to zero when out of this position. I need scarcely add, that the direction of the deviation corresponded with that of the calorific action.

The experiment was therefore perfectly clear, and could leave no shadow of doubt; indeed, I had occasion to repeat it more recently in the presence of M. Belli, professor of physics at the university of Pavia; of MM. Mossatti and Lavagna, of the university of Pisa; and of many other distinguished *savans*, who all left my study perfectly satisfied that the light of the moon is calorific.

When I reflect, that the philosophers who have attempted to discover lunar heat, in the course of the last century, employed, after the manner of Laland, lenses of 1 metre, and 1.33 m. in diameter, and the extremely sensible thermoscope of Amontons, I strongly suspect that the negative results announced by those philosophers were due in great part to the cold generated in their lenses by celestial radiation, joined with the cooling caused by the agitations of the outer air to which their instruments were exposed; so that I do not at all despair of being able to render the phenomenon apparent with the thermoscope by ordinary expansion.

Meanwhile, by the employment of my present means of

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observation, I have been able to satisfy myself that the calorific action of the moon varies, as might well be expected, not only with its age, but also with the height of this planet above the horizon. A small deviation of the plane of the lens from the direction normal to the rays, considerably diminishes the effect. Under these different circumstances, I obtained deviations that varied from 0°.6 to 4°.8. The action through the glasses is made in a manner so slowly gradual, that the index of the apparatus moves with an admirable regularity, and, without undergoing the slightest oscillation either in going out of its position of equilibrium when the thermoscopic body is placed in the focus of the lens, or in returning to it when this body is drawn just out of the focus, still maintaining it facing the lens. Accomplished under different lunations, *the experiment always succeeded;* namely, the result was more or less pronounced, but *always indicating an increase of temperature.* I will therefore repeat, that the fact of the existence of heat in the radiation of the moon is perfectly sure: the only question now is to measure this calorific action, and to see, first, what its value is in thermometric degrees; 2nd, what its relation is to solar radiation. I will endeavour to solve these two questions; but, in reference to the latter, under what degree of approximation can we consider the fraction $\frac{1}{300000}$, given by Bouguer, to represent the relation of the luminous intensity of the moon to that of the sun?—[A.]

CLVIII. *On the Magnetization of Light, and the Illumination of Magnetic Lines of Force.* By Michael Faraday, Esq., D.C.L., F.R.S. &c. (From Phil. Trans. Part I., for 1846. Read Nov. 20th, 1845.)

(Abstract.)

These researches have already been reported to our readers at some length; but now, with the papers themselves before us, we shall enter more into detail: the following note is added to the title:—

"The title of this paper has, I understand, led many to a misapprehension of its contents, and I, therefore, take the liberty of appending this explanatory note. Neither accepting nor rejecting the hypothesis of an ether, or the corpuscular, or any other view that may be entertained of the nature of light; and, as far as I can see, nothing being really known of a ray of light more than of a line of magnetic or electric force, or even of a line of gravitating force, except as it and they are

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manifest in and by substances; I believe that in the experiments I describe in the paper, light has been magnetically affected, *i. e.* that that which is magnetic in the forces of matter has been affected, and in turn has affected that which is truly magnetic in the force of light; by the term magnetic, I include here either of the peculiar exertions of the power of a magnet, whether it be that which is manifest in the magnetic or the diamagnetic class of bodies. The phrase 'illumination of the lines of magnetic force' has been understood to imply that I had rendered them luminous. This was not within my thought. I intended to express that the line of magnetic force was illuminated as the earth is illuminated by the sun, or the spider's web illuminated by the astronomer's lamp. Employing a ray of light, we can tell *by the eye* the direction of the magnetic lines through a body; and by the alteration of the ray and its optical effect on the eye, can see the course of the lines just as we can see the course of a thread of glass or any other transparent substance, rendered visible by the light; and this was what I meant by illumination, as the paper fully explains."

The author at the outset repeats his opinion of the common origin of the forces of matter, or, in other words, their convertibility, and alludes to the accumulation of proof in modern times. He adverts to the negative results of his former attempts to discover a direct relation between light and electricity, and to Wartmann's confirmation of the same.* Still persuaded that such relation did exist, he resumed his inquiries, and has now succeeded in *magnetising and electrifying a ray of light, and in illuminating a magnetic line of force.* By *line of magnetic force,* or *magnetic line of force,* or *magnetic curve,* he means that exercise of magnetic force which is exerted in the lines usually called magnetic curves, which exist equally from and to magnetic poles, and to circles about an electric current. By *line of electric force,* he means the force exerted in the lines joining two bodies, active according to the laws of static induction. By *diamagnetic,* a body through which lines of magnetic force pass, without its assuming the magnetic state of iron or loadstone.

A ray of light, polarized by reflection, passes through a Nichol's eye-piece; between the mirror and the eye-piece were the poles either of a horse-shoe or of two cylinder electro-magnets, so placed that the *magnetic lines of force* were parallel with the ray: so that any transparent substance placed between the two poles would have the polarized ray

* *Archives de l'Elect.* vol. ii., p. 596, 600.

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and the magnetic lines passing through it in the same direction. A heavy glass, prepared by the author sixteen years ago, and from its constituents called silicated borate of lead, was the first body which gave the discovery of these relations between light and magnetism.

A piece of this two inches square and half an inch thick was placed between the poles not yet magnetised: the polarized ray passed through it, but it had no effect on the ray. The eye piece was turned till the ray became invisible, and the electric current was laid on, when the ray immediately became visible; and disappeared when the current ceased. Five pairs of Grove's were used; and each pole of the electro-magnets would singly sustain from 28 to 56 or more pounds.* The character of the force is that of *rotation*. The following are the laws of the rotation:—

"When the pole nearest to the observer was a marked pole, *i. e.* the same as the north end of a magnetic needle, and the further pole was unmarked, the rotation of the ray was right handed; for the eyepiece had to be turned to the right hand, or clock fashion, to overtake the ray and restore the image to its first condition. When the poles were reversed, which was instantly done by changing the direction of the electric current, the rotation was changed also, and became left-handed, the alteration being to an equal degree in extent as before. The direction was always the same for the same line of magnetic force."

The same results were obtained, although feebly, with a common steel magnet. The ray was passed through two hollow iron electro-magnetic cores, with the diamagnetic between: the effect was the same.

"One magnetic pole only was used, that being one end of a powerful cylinder electro-magnet. When the heavy glass was beyond the magnet, being close to it, but between the magnet and the polarizing reflector, the rotation was in one direction dependent on the nature of the pole; when the diamagnetic was on the near side being close to it, but between it and the eye, the rotation for the same pole was in the contrary direction to what it was before; and when the magnetic pole was changed, both these directions were changed with it. When the heavy glass was placed in a corresponding position to the pole, but above or below it, so that the magnetic curves were no longer passing through the glass

* Mr. Faraday mentions Mr. Anderson: he says, "He has assisted me in all the researches into which I have entered since that time, and to his care, steadiness, exactitude, and faithfulness in the performance of all that has been committed to his charge I am much indebted."

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parallel to the ray of polarized light, but rather perpendicular to it, then no effect was produced.

"They give the diamagnetic the power of rotating the ray; and the law of this action on light is, that if a magnetic line of force be going from a north pole, or coming from a south pole, along the path of a polarized ray coming to the observer, it will rotate that ray to the right hand; or that if such a line of force be coming from a north pole, or going from a south pole, it will rotate such a ray to the left hand. If a cork or cylinder of glass representing the diamagnetic be marked at its ends with the letters N. and S. to represent the poles of a magnet, the line joining these lettters may be considered as a magnetic line of force; and further, if a line be traced round the cylinder with arrow heads on it to represent direction, such a simple model held up before the eye will express the whole of the law, and give every portion and consequence of direction resulting from it.

"If a watch be considered as the diamagnetic, the north pole of a magnet being imagined against the face, and a south pole against the back, then the motion of the hands will indicate the direction of rotation which a ray of light undergoes by magnetisation."

The rotation is greater in proportion to the extent of diamagnetic traversed by the ray and the magnetic lines of force; this was proved by interposing various specimens of the glass, other things remaining the same: increase of width of the diamagnetic has no effect. The rotation *increases* with the intensity of the magnetic lines of force; and apparently in proportion.

Other bodies, besides heavy glass, are thus active; and when these have a rotative power of their own, the effect of the magnetic force is to add to or subtract from their specific force. No perceptible change in the character of these actions was observed by any degree of motion that was given to the diamagnetic. The interposition of copper, lead, tin, silver, and other non-magnetic bodies in the course of the magnetic curves produced no interference. Iron frequently produced considerable results: it either altered the direction of the magnetic lines, or disposed within itself of their force.

"Thus, when two contrary poles were on one side of the polarized ray, and the heavy glass in its best position between them and in the ray, the bringing of a large piece of iron near to the glass on the other side of the ray caused the power of the diamagnetic to fall. This was because certain lines of magnetic force, which at first passed through the glass parallel

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to the ray, now crossed the glass or the ray; the iron giving two contrary poles, opposite the poles of the magnet, and thus determining a new course for a certain portion of the magnetic power, and that across the polarized ray.

"Or, if the iron, instead of being applied on the opposite side of the glass, were applied on the same side with the magnet, either near it or in contact with it, then, again, the power of the diamagnetic fell, simply because the power of the magnet was directed from it in a new direction. This effect depends much, of course, on the intensity and power of the magnet, and the size and softness of the iron."

When the iron cores were removed from the electro-helices, the effects were very feeble, which shows "that the phenomena exhibited by light under these circumstances is directly connected with the magnetic forms of force supplied by the arrangement."

"Another effect which occurred illustrated the same point. When the contact at the voltaic battery is made, and the current sent round the electro-magnet, the image produced by the rotation of the polarized ray does not rise up to its full lustre immediately, but increases for a couple of seconds, gradually acquiring its greatest intensity: on breaking the contact it sinks instantly, and disappears apparently at once.

"The gradual rise in brightness is due to the "time" which the iron core of the magnet requires to evolve all that magnetic power which the electric current can develope in it; and, as the magnetism rises in intensity, so does its effect on the light increase in power: hence the progressive condition of the rotation."

The author could not discover any specific magnetic-inductive action in heavy glass thus circumstanced. When water, alcohol, &c., were made diamagnetics, no difference of volume occurred when the magnetic curve passed through them.

If all transparent bodies possess the power above described, they have it in very different degrees: some have not yet shown any. Bodies differing much in physical character equally possess this power;—"for solids and liquids, acids, alkalies, oils, water, alcohol, ether, all possess the power." And, though the *degree* of action may differ in all, it is the same in *kind*—namely, a rotative power; and the direction is not dependent on the nature of the diamagnetic, but upon the direction of the magnetic curves, according to the law enunciated above. The *silico-borate of lead* is eminently fitted

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for the experiments, but it must be well annealed. The *borate of lead* gives equally good results; and, being more fusible, is mere manageable. *Flint-glass* exhibits the phenomena in a less degree; and *crown-glass* in a still less. Crystalline bodies as a class seemed to "resist the assumption of the rotating state." *Rock-salt* and *fluor-spar* were slightly active. *Alum* seemed to be so. *Rock-crystal*, four inches across, and other smaller specimens, gave negative results—as did also *Iceland spar*, *sulphate of lime*, and *carbonate of soda*. If *ice* gave a result, it was due to the fusion of its surface. *Gold-leaf* was tried with curiosity: no effect. These limited dimensions of the path traversed by the ray scarcely gave hope of a result.

"In experiments with liquids a very good method of observing the effect is to inclose them in bottles from 1½ to 3 or 4 inches in diameter, placing these in succession between the magnetic poles, and bringing the analysing eye-piece so near to the bottle, that, by adjustment of the latter, its cylindrical form may cause a diffuse but useful image of the lamp's flame to be seen through it. The light of this image is easily distinguished from that which passes by irregular refraction through the striæ and deformations of the glass, and the phenomena being looked for in this light are easily seen."

Water, alcohol, ether, all the fixed oils that were tried—the essential oils, naphtha, spermaceti, fused sulphur—the chloride of sulphur and of arsenic; and, in fact, every liquid substance tried gave it. One hundred and fifty different aqueous solutions were tried—their exceeding diversity causes no exception to the rule—all gave it. All the gases that were tried gave negative results.

"I may record that the following are the substances submitted to experiment:—Castor oil, resin oil, oil of spike, of lavender, of laurel, Canada balsam, alcoholic solution of camphor, alcoholic solution of camphor and corrosive sublimate, aqueous solutions of sugar, tartaric acid, tartrate of soda, tartrate of potassa and antimony, tartaric and boracic acid, and sulphate of nickel, which rotated to the right hand; copaiba balsam, which rotated the ray to the left hand; and two specimens of camphine or oil of turpentine, in one of which the rotation was to the right hand, and in the other to the left. In all these cases, as already said, the superinduced magnetic rotation was according to the general law, and without reference to the previous power of the body."

Melted camphor had high natural power, but the magnetic curves added nothing to it.

Considering the relation between a magnet and the electric

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current, the author expected that the latter would have a similar action on light; "and in the helix would supply a form of apparatus, in which great lengths of diamagnetics" might be examined. A *long helix* was formed of 1240 feet of 0·03 inches copper wire: it was 0·4 inches external diameter, and 65 inches long. A *medium helix* had 80 feet of 0·2 inches copper wire: it was 1·87 inches diameter, and 19 inches long. In each case, the wire was in two superposed spirals. The third, or Woolwich helix, which had been previously made under Prof. Faraday's direction, for Lieut.-Col. Sabine, at Woolwich, had 501 feet of 0·17 inches wire: it was 2·5 inches internal, and 4·75 external diameter, and 26·5 inches long. The wire was in four superposed spirals. The long helix acted feebly, the medium less so, and the Woolwich very strongly on a compass needle placed near it. Solid bodies were cut into form and placed as cores in these helices; liquids were similarly placed in tubes, with flat glass ends, attached by Canada balsam for aqueous fluids, and gum for alcohol, ether, &c.

"The general effect produced by this form of apparatus may be stated as follows:—The tube within the long helix was filled with distilled water and placed in the line of the polarized ray, so that, by examination through the eye-piece, the image of the lamp flame produced by the ray could be seen through it. Then the eye-piece was turned until the image of the flame disappeared, and afterwards the current of ten pairs of plates sent through the helix; instantly the image of the flame reappeared, and continued as long as the electric current was passing through the helix: on stopping the current, the image disappeared. The light did not rise up gradually, as in the case of electro-magnets, but instantly. These results could be produced at pleasure. In this experiment we may, I think, justly say that a ray of light is electrified, and the electric forces illuminated."

Doubtful cases are assisted by a lens of long focus between the tube and mirror, or of short focus between the tube and the eye. In cases of feeble manifestation it is better not quite but *nearly* to extinguish the ray by the rotation of the eyepiece; and then the action of the current either quite extinguishes the ray or else re-illuminates it.

"When the current was sent round the helix in one direction, the rotation induced upon the ray of light was one way; and when the current was changed to the contrary direction, the rotation was the other way. In order to express the direction, I will assume, as is usually done, that the current passes from the zinc through the acid to the platinum in the same cell: if

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such a current pass under the ray towards the right upwards on its right side, and over the ray towards the left, it will give left-handed rotation to it; or if the current pass over the ray to the right, down on the right side, and under it towards the left, it will induce it to rotate to the right hand.

"The law, therefore, by which an electric current acts on a ray of light is easily expressed. When an electric current passes round a ray of polarized light in a plane perpendicular to the ray, it causes the ray to revolve on its axis as long as it is under the influence of the current, in the *same direction* as that in which the current is passing."

With respect to the degree of action, the greater the column of water subjected to the action of the helix, the greater the rotation; and it seemed to be in direct proportion to the length. If a short tube of water on a piece of heavy glass were in the Woolwich helix, the effect was the same, whatever place it occupied, provided it was within the helix; so that every part of the helix appears to exercise the same action. When a long tube of water of small diameter was placed within the helix, the effect was the same whether it were in the axis or on one side of it. So also, when a ray passed through a large tube of water, the effect was the same whether it traversed the axis of the tube (which was also the axis of the helix), or was on the side of the axis. Bodies possessing natural rotative power when introduced into the helix have the additional rotative power superinduced upon them. A helix of uncovered copper-wire was placed in water:—when the current was sent *through* it, the water *within* the helix evinced rotative power; but not the slightest trace could be detected outside. The water was inclosed in brass or copper tubes, and at times these were surrounded with an *iron* tube, and within the helix: the effect was the same. An iron bar and a water tube were placed in the helix: the effect was the same. A small iron tube with glass-ends, filled with water, was placed in the helix, and showed a certain rotative power: a second iron tube was placed over the former, the rotation *increased:* a third was placed over the former tube, the rotation fell.

"These results are complicated, being dependent on the new condition which the character of iron gives to its action on the forces. Up to a certain amount, by increasing the development of magnetic forces, the helix and cone, *as a whole*, produce increased action on the water; but on the addition of more iron and the disposal of the forces through it, their action is removed in part from the water, and the rotation is lessened."

All bodies subjected to the helix acted just as they did

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when subjected to magnetism. *Air* was examined with much anxiety, but no effect followed. In these experiments the helix must be so arranged as not to raise the temperature of the water, otherwise the results will be vitiated. And as it is almost an impossibility to obtain a piece of glass that does not depolarize, the parts that do not depolarize must always be referred to; and the eye must be brought near the glass.

As a general indication of the relative amount of induced rotation, and on the assumption that the power was proportionate to the length of the rays, taking water at 1, the following numbers were obtained:—

Oil of turpentine	11.8
Heavy glass	6.0
Flint glass	2.8
Rock salt	2.2
Water	1.0
Alcohol	less than water.
Ether	less than alcohol.

As it adds to our knowledge to be made acquainted with the cases in which no effects obtain, the following negative results are given:—A polarized ray was passed through heavy glass, flint glass, rock crystal, Iceland spar, oil of turpentine, and air; lines of electro-static tension, from the Leyden jar, or electrical machine, were made to pass parallel, as in the other cases;—no effect. The tension of a secondary current produced no effect. Electro-static and magnetic lines were made to act; the effect was the same as if the former had not been there. The polarised ray and electric currents were passed in every possible way through electrolytes, but without effect. The electrolytes employed were distilled water, solution of sugar, dilute sulphuric acid, solution of sulphate of soda, and of sulphate of copper. Electric currents and magnetic lines were passed simultaneously through dilute sulphuric acid or sulphate of soda; but the results were negative, except in so far as the magnetic lines were concerned. "In one arrangement, the current passed in the direction of radii from a central to a circumferential electrode, the contrary magnetic poles being placed above and below; and the arrangements were so good that, when the electric current was passing, the fluid rapidly rotated; but a polarized ray sent horizontally across this arrangement was not at all affected:" nor was it when sent vertically.

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Having mentioned that thus, "for the first time,* a true direct relation and dependence between light and the electric and magnetic forces" is established, the author adds:—

"I ought, perhaps, rather to say that another form of the great power is distinctly and directly related to the other forms; or, that the great power manifested by particular phenomena in particular forms, is here further identified and recognized by the direct relation of its form of light to its forms of electricity and magnetism."

This relation between magnetism and *polarized* light is described as being more interesting than if it had been shown to exist with mere common light: and Mr. Faraday says that it cannot but extend to common light. The magnetic lines of force *alone* act on the rays of light, and *only* when they are parallel or nearly so with these rays; and they do not act directly but by the intervention of the matter in which they and the ray have a common existence—which is proved by the inaction of a vacuum and of air or gases; and further, by the special degree of action peculiar to different bodies. That the magnetic force acts on light, shows that there is a direct relation between them; and that *substances* are necessary to develope the action, shows that it cannot exist but through the intervention of matter.

"Recognising or perceiving 'matter' only by its powers, and knowing nothing of any imaginary nucleus abstract from the idea of these powers, the phenomena described in this paper much strengthen my inclination to trust in the views I have on a former occasion advanced in reference to its nature."†

It, of course, cannot be doubted that the magnetic forces affect the molecular constitution of matter as much in the dark as in light; but light appears, as yet, the only means of observing this change, and this change must also occur in opaque bodies; for there is no distinction between them or diamagnetics. So that transparency is only a distinction between individuals of the class.

"If the magnetic forces had made these bodies magnets, we could by light have examined a transparent magnet; and that would have been a great help to our investigation of the forces of matter. But it does not make them magnets; and therefore the molecular condition of these bodies, when in the

* In a note, the author says, in reference to Morrichim's magnetizing a needle by the violet ray, "that when it has occurred, it has been secondary, incidental, and perhaps even accidental:" he, when in Italy with Davy, in 1814, could not produce it.

† Vide *Elec. Mag.* Vol. I. p. 295.

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state described, must be specifically distinct from that of magnetized iron, or other such matter, and must be a *new magnetic condition;* and as the condition is a state of tension (manifested by its instantaneous return to the normal state when the magnetic induction is removed), so the *force* which the matter in this state possesses, and its mode of action, must be to us a 'new magnetic force or mode of action' of matter."

For it is impossible not to recognise in the hard glass, or water, &c., under the influence of magnetism, properties *new* to the substance, and subject to very definite laws.

"Perhaps this state is a state of *electric tension, tending to a current;* as in magnets, according to Ampère's theory, the state is a state of *current.* When a core of iron is put into a helix, everything leads us to believe that currents of electricity are produced within it, which rotate or move in a plane perpendicular to the axis of the helix. If a diamagnetic be placed in the same position, it acquires power to make light rotate in the same plane. The state it has received is a state of tension, but it has not passed on into currents, though the acting force, and every other circumstance and condition, are the same as those which do produce currents in iron, nickel, cobalt, and such other matters as are fitted to receive them. Hence the idea, that there exists in diamagnetics under such circumstances a tendency to currents, is consistent with all the phenomena as yet described; and is further strengthened by the fact, that, leaving the loadstone or the electric current, which by inductive action is rendering a piece of iron, nickel, or cobalt magnetic, perfectly unchanged, a mere change of temperature will take from these bodies their extra power, and make them pass into the common class of diamagnetics."

Mentioning that this is the first time that the molecular condition, requisite for polarization, has been artificially given, the author adds:—

"At first one would be inclined to conclude that the natural state and the state conferred by magnetic and electric forces must be the same, since the effect is the same; but, on further consideration, it seems very difficult to come to such a conclusion. Oil of turpentine will rotate a ray of light, the power depending upon its particles, and not upon the arrangement of the mass. Whichever way a ray of polarized light passes through this fluid, it is rotated in the same manner; and rays passing in every possible direction through it 'simultaneously,' are all rotated with equal force and accord-

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ing to one common law of direction; *i. e.* either all right-handed, or else all to the left. Not so with the rotation superinduced on the same oil of turpentine by the magnetic or electric forces; it exists only in one direction, *i. e.* in a plane perpendicular to the magnetic line; and being limited to this plane, it can be changed in direction by a reversal of the direction of the inducing force. The direction of the rotation produced by the natural state is connected invariably with the direction of the ray of light; but the power to produce it appears to be possessed in every direction, and at all times, by the particles of the fluid: the direction of the rotation produced by the induced condition is connected invariably with the direction of the magnetic line or the electric current, and the condition is possessed by the particles of matter, but strictly limited by the line or the current changing and disappearing with it."

If a ray proceed from *a* to *b* through oil of turpentine, and the eye be at *b*, the rotation is right-handed; if the ray pass from *b* to *a*, and the eye be at *a*, the rotation is also right-handed: but if an electric current be passed round in one direction, and a ray pass from *a* to *b*, the eye at *b* will see a greater right-handed rotation; but pass the ray from *b* to *a*, and look at the *a* end, and the rotation is left-handed. "Hence the particles of fluid that rotate by virtue of their natural force, and those that rotate by virtue of the induced force, cannot be in the same condition." These differences may eventually be harmonised; in the mean time they open out new paths of inquiry. Were it not for these differences, we might see an analogy between bodies always possessing rotative power and those to which this power is given by magnetism; or between a natural magnet and a helix. But "the natural condition of the magnet and quartz, and the constrained condition of the helix and heavy glass, form the link of analogy in one direction; whilst the supposition of currents existing in the magnet and helix, and only a tendency or tension to currents existing in the quartz and heavy glass, supplies the link in the transverse direction."

Two classes of bodies here give no indication of this power over light—gases and vapours; and certain crystalline bodies, as rock crystal, &c. With regard to the latter, proofs of another kind will be given in the following series of researches that they do acquire the power; and with regard to the former, the author believes they do assume the state, although in too minute a degree to be detected. In the mean time, we must not assume that bodies carry their properties into the gaseous state, which belong to them as solids. Equiaxial and singly

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refracting crystals have affected the ray; unequiaxial and doubly refracting have not; which may even indicate a relation between magnetism, electricity, and crystallizing forces.

As helices and magnets act in the same manner on bodies traversed by light, we have another proof of Ampère's theory. In the theory of static induction as an action of contiguous particles, which the author published some years ago, he suspected a similar property in magnetic action, but "could not then discover any peculiar condition of the intervening substance or diamagnetic; but now" (he adds) "that I have been able to make out such a state, which is not only a state of tension, but dependent entirely upon the magnetic lines which pass through the substances, I am more than ever encouraged to believe that the view then advanced is correct." The author thinks that the power of magnetism on common light "probably will soon be made apparent by experiment;" and also that the same kind of action may take place on other radiant agents, as heat and chemical force. He concludes thus:—"What the possible effect of the force may be in the earth, as a whole, or in magnets, or in relation to the sun, and what may be the best means of causing light to evolve electricity and magnetism, are thoughts continually pressing upon the mind:" and adds, that it is, however, better to search for real truth than to invent suppositions. The paper is dated Oct. 29, 1845.

CLIX. (II.) *On New Magnetic Actions, and on the Magnetic Condition of all Matter.* By Prof. FARADAY. Read before Royal Society, Dec. 18, 1845.

(ABSTRACT.)

THE author has appended a note to the title of this paper, saying that Prof. Wheatstone had just mentioned to him a paper by M. Becquérel, in which the action of a magnet on the deutoxide and tritoxide of iron, and on a needle of wood, is described as placing them *across* the magnetic curves; and of which the author says:—"This difference of effect, which establishes a line of demarkation between these two species of phenomena, is due to this, that the magnetism being very feeble in the tritoxide of iron, wood, &c., we may neglect the reaction of the body on itself; and, therefore, the direct action of the bar ought to overrule it." But as this paper makes all magnetic action of one kind; and as Mr. Faraday has shown that there are two *distinct* kinds of such action, he does not see a reason to alter a word or the date of what he had written.

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The phenomena now to be discussed are quite new. They require very powerful magnets. One of those used was the Woolwich helix already described, which could sustain one or two cwt., when excited with 10 pairs of Groves.

"Another magnet," says the author, "which I have had made has the horse-shoe form. The bar of iron is 46 inches in length and 3·75 inches in diameter, and is so bent that the extremities forming the poles are six inches from each other; 522 feet of copper wire, 0·17 of an inch in diameter, and covered with tape, are wound round the two straight parts of the bar, forming two coils on these parts, each sixteen inches in length, and composed of three layers of wire; the poles are of course six inches apart, the ends are planed true, and against these move two short bars of soft iron, 7 inches long and 2½ by 1 inch thick, which can be adjusted by screws and held at any distance less than six inches from each other. The ends of these bars form the opposite poles of contrary name; the magnetic field between them can be made of greater or smaller extent, and the intensity of the lines of magnetic force be proportionately varied."

The substances were suspended by a stirrup of paper attached to a strand of several pieces of cocoon silk; or by a cradle of copper wire attached to a fine thread six feet long. A glass cylinder shielded the substances from motion by currents of air. The materials of the apparatus are first examined to see that they are void of magnetism; for it is difficult to find a piece of paper for a stirrup that is not somewhat magnetic. The substances to be experimented with are examined, and rejected if found magnetic. The author adds:—

"I shall have such frequent occasion to refer to two chief directions of position across the magnetic field, that to avoid periphrasis, I will here ask leave to use a term or two conditionally. One of these directions is that from pole to pole, or along the line of magnetic force; I will call it the axial direction. The other is the direction perpendicular to this, and across the line of magnetic force; and for the time, and as respects the space between the poles, I will call it the equatorial direction. Other terms that I may use I hope will explain themselves."

When the bar of heavy glass before mentioned was suspended between the poles of the magnet, and the current was laid on, it moved round the point of suspension and placed itself across the magnetic curve; if moved, it returned: either end of the bar was indifferent as to which side it went. The bar always went by the shortest course to the equatorial position.

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"There are two positions of equilibrium for the bar; one stable the other unstable. When in the direction of the axis or magnetic line of force, the completion of the electric communication causes no change of place; but if it be the least oblique to this position, then the obliquity increases until the bar arrives at the equatorial position; or if the bar be originally in the equatorial position, then the magnetism causes no further changes, but retains it there."

If the bar is suspended nearer to one pole than to the other, it still takes the position perpendicular to the magnetic lines; but it also recedes from the nearer pole and remains so until the battery is cut off. Either pole has this effect; but, when the bar is equidistant, no repulsion occurs. If the bar is suspended in the equatorial line, but a little distance from the axial line, it takes the usual direction, but it is also repelled further from the axial line.

"Instead of two magnetic poles, a single pole may be used, and that either in a vertical or horizontal position. The effects are in perfect accordance with those described above; for the bar when near the pole is repelled from it in the direction of the line of magnetic force, and at the same time it moves into a position perpendicular to the direction of the magnetic lines passing through it. When the magnet is vertical to the bar by its side, this action makes the bar a tangent to the curve of its surface."

The glass cannot point unless of an elongated form; but if two or three rounded pieces be placed side by side in a paper tray, they point. Portions, however, of any form are *repelled;* so if two pieces are hung in the axial line, one near each pole, they will be repelled by the respective poles, and so seem to attract each other; or, if hung in the equatorial line, one on each side of the axis, they appear to repel. In fact the particle would move along the magnetic curves in either direction, the only constant point being that it would move from stronger to weaker places.

"The cause of the pointing of the bar or any oblong arrangement of the heavy glass is now evident. It is merely a result of the tendency of the particles to move outwards, or into positions of weakest magnetic action. The joint exertion of the action of all the particles brings the mass into the position which by experiment is found to belong to it."

When one or two magnetic poles are active at once, certain curves are formed, which the author terms diamagnetic curves. The same happens to the bar, if suspended in water, as in air; and equally in vessels of wood, stone, earth, copper, lead,

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silver, and other diamagnetics. The same effects have been obtained by a good common steel horse-shoe magnet, but not by helices without iron cores.

"Here, therefore, we have magnetic repulsion without polarity—*i. e.*, without reference to a particular pole of the magnet, for either pole will repel the substance, and both poles will repel it at once.

"The heavy glass, though subject to magnetic action, cannot be considered as magnetic, in the usual acceptation of that term, or as iron, nickel, cobalt, and their compounds. It presents to us, under these circumstances, a magnetic property new to our knowledge; and though the phenomena are very different in their nature and character to those presented by the action of the heavy glass on light, still they appear to be dependent or connected with the same condition of the glass as made it then effective, and therefore, with those phenomena, prove the reality of this new condition."

Bodies active by magnetism on a polarized ray, and others which cannot from their opacity be examined by a polarized ray, are now studied. Borate of lead, flint and crown glass, acted in a similar manner; also phosphorus, sulphur, India rubber; as well as crystalline bodies both of the single and of the double refracting class. He continues:—

"I then proceeded to subject a great number of bodies, taken from every class, to the magnetic forces, and will, to illustrate the variety of the nature of the substances, give a comparatively short list of crystalline, amorphous, liquid, and organic bodies below. When the bodies were fluids I inclosed them in thin glass tubes. Flint glass points equatorially, but if the tube be of very thin glass, this effect is found to be small when the tube is experimented with alone; afterwards, when it is filled with liquid and examined, the effect is such that there is no fear of mistaking that due to the glass for that of the fluid. The tubes must not be closed with cork, sealing-wax, or any ordinary substance taken at random, for these are generally magnetic. I have usually so shaped them in the making, and drawn them off at the neck, as to leave the aperture on one side, so that when filled with liquid they required no closing.

Rock crystal	Muriate of ammonia
Sulphate of lime	Chloride of lead
Sulphate of baryta	Chloride of sodium
Sulphate of soda	Nitrate of potassa
Sulphate of potassa	Nitrate of lead
Sulphate of magnesia	Carbonate of soda
Alum	Iceland spar

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Acetate of lead	Casseine
Tartrate of potash and antimony	Cinchonia
	Margaric acid
Tartrate of potash and soda	Wax from shell-lac
Tartaric acid	Sealing-wax
Citric acid	Olive oil
Water	Oil of turpentine
Alcohol	Jet
Ether	Caoutchouc
Nitric acid	Sugar
Sulphuric acid	Starch
Muriatic acid	Gum arabic
Solutions of various alkaline and earthy salts	Wood
	Ivory
Glass	Mutton, dried
Litharge	Beef, fresh
White arsenic	Beef, dried
Iodine	Blood, fresh
Phosphorus	Blood, dried
Sulphur	Leather
Resin	Apple
Spermaceti	Bread."

The author remarks how strange it is to find a piece of wood, beef, or apple obeying a magnet; and says that if a man were suspended with sufficient delicacy, he would point equatorially. Comparatively large masses would point equally well with small ones; because in larger masses more lines of magnetic force act upon the body. Flat plates or rings were quite as good as cylinders. A plate or ring of glass floated on water and free to move, and subject to magnetic forces diminishing in intensity would set itself equatorially.

"I do not find that division of the substance has any distinct influence on the effects.

"A piece of Iceland spar was observed as to the degree of force with which it set equatorially; it was then broken into six or eight fragments, put into a glass tube, and tried again; as well as I could ascertain, the effect was the same. By a second operation, the calcareous spar was reduced into coarse particles, afterwards to a coarse powder, and ultimately to a fine powder; being examined as to the equatorial set each time, I could perceive no difference in the effect, until the very last, when I thought there might be a slight diminution of the tendency, but if so it was almost insensible. I made the experiment on silica with the same result of no diminution of power. In reference to this point I may observe that starch and other bodies in fine powder exhibited the effect very well."

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The author had made but little progress in examining the specific degree of this power of magnetic action : heavy glass stands above flint glass, and this above plate glass. Water is below these; alcohol below water; and ether lower still. Phosphorus appears above all bodies; and its direction was shown even by a common magnet.

"I was much impressed by the fact that blood was not magnetic, nor any of the specimens tried of red muscular fibre of beef or mutton. This was the more striking, because, as will be seen hereafter, iron is 'always'—and in almost 'all states'—magnetic. But, in respect to this point, it may be observed, that the ordinary magnetic property of matter and this 'new property' are in their effects opposed to each other; and that when this property is strong, it may overcome a very slight degree of ordinary magnetic force—just as also a certain amount of the magnetic property may oppose, and effectually hide, the presence of this force. It is this circumstance which makes it so necessary to be careful in examining the magnetic condition of the bodies in the first instance. The following list of a few substances, which were found slightly magnetic, will illustrate this point:—Paper, sealing-wax, china ink, Berlin porcelain, silkworm gut, asbestos, fluor-spar, red-lead, vermilion, peroxide of lead, sulphate of zinc, tourmaline, plumbago, shell-lac, charcoal. In some of these cases the magnetism was generally diffused through the body; in other cases it was limited to a particular part. Having arrived at this point, I may observe that we can now have no difficulty in admitting that the phenomena abundantly establish the existence of a magnetic property in matter, new to our knowledge. Not the least interesting of the consequences that flow from it, is the manner in which it disposes of the assertion which has sometimes been made—that all bodies are magnetic. Those who hold this view, mean that all bodies are magnetic as iron is, and say that they point between the poles. The new facts give not a mere negative to this statement, but something beyond, namely, an affirmative as to the existence of forces in all ordinary bodies, directly the opposite of those existing in magnetic bodies; for whereas those practically produce attraction, these produce repulsion; those set a body in the axial direction, but these make it take up an equatorial position; and the facts with regard to bodies generally are exactly the reverse of those which the view quoted indicates."

The metals might, from their peculiar relation to electric and magnetic forces, have been expected to present some peculiar phenomena in respect to these new actions: not so, however. It was a point of great interest to see whether, as a mass, they

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could be generalized, or could be classified into groups. The metals were first examined in search of ordinary magnetism, and this examination required magnets of the power described for these experiments; for although specimens would present no indications in presence of a compass needle or an ordinary magnet, yet abundant signs were given when the large magnets were applied.

"My test of magnetism was this:—If a bar of the metal to be examined, about two inches long, was suspended in the magnetic field, and, being at first oblique to the axial line, was, upon the supervention of the magnetic forces, drawn into the axial position instead of being driven into the equatorial line or remaining in some oblique position, then I considered it magnetic: or if, being near one magnetic pole, it was attracted by the pole instead of being repelled, then I considered it was magnetic. It is evident that the test is not strict; because, as before pointed out, a body may have a slight degree of magnetic force, and yet the power of the new property be so great as to neutralize or surpass it. In the first case, it might seem neither to have one property nor the other; in the second case, it might appear free from magnetism and possessing the special property in a *small* degree."

Antimony, bismuth, cadmium, copper, gold, lead, mercury, silver, tin, and zinc, were, with these limitations, non-magnetic. Platinum, paladium, and titanium were magnetic, in common with iron, nickel, and cobalt. Whether some of the phenomena presented by the various metals be due or not to the presence of iron is not yet determined. In reference to the non-magnetic metals, there are two sets of effects to be distinguished from each other;—one depends on induced magnetic currents, and the other on this new property. Non-magnetic metals produce all the effects already described for heavy glass; bismuth appears better than the heavy glass, and even than phosphorus.

"To speak accurately, the bismuth bar which I employed was two inches long, 0.33 of an inch wide, and 0.2 of an inch thick. When this bar was suspended in the magnetic field between the two poles, and subject to the magnetic force, it pointed freely to the equatorial direction as the heavy glass did; and if disturbed from that position, returned *freely* to it. This latter point, though perfectly in accordance with the former phenomena, is in striking contrast with the phenomena presented by copper and some other of the metals, as to require no particular notice here."

The extreme sensibility of bismuth gives the power of analyzing several apparently complex actions; all of which, how-

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ever, are reduced to the one general law. Bismuth points well when immersed in water, alcohol, ether, oil, mercury, &c., or enclosed in non-magnetic metal vessels, or behind similar metal screens; and this occurs whether it be in mass or in powder. Two pieces of bismuth simultaneously acted upon by the magnet have no mutual action. Powdered bismuth sprinkled on paper over the circular pole of the magnet retreated, leaving a blank circle with the particles outside tending to separation: over a conical pole, when the paper of filings was drawn along, the pole traced out a line. The following order may give an idea of the relative places of the non-magnetic metals:—Bismuth, antimony, zinc, tin, cadmium, mercury, silver, copper.

Faraday had a vague idea that the repulsion of bismuth had been observed before; if so, what was then an isolated fact is now shown to be a general property. Since writing the above, M. de la Rive pointed out that it was observed sixteen years ago by La Baillif, of Paris. In respect to this, Faraday says:—

"M. La Baillif showed sixteen years ago that both bismuth and antimony repelled the magnetic needle. It is astonishing that such an experiment has remained so long without further results. I rejoice that I am able to insert this reference before the present series of these researches goes to press. Those who read my papers will see here, as on many other occasions, the results of a memory which becomes continually weaker; I only hope that they will be excused, and that omissions and errors of that nature will be considered as involuntary."

He now examines some peculiar effects presented by copper and some other metals, which must be borne in mind, or they will lead to doubt and confusion.

"If, instead of a bar of bismuth, a bar of copper of the same size be suspended between the poles, and magnetic power be developed whilst the bar is in a position oblique to the axial and equatorial lines, the experimenter will perceive the bar to be affected; but this will not be manifested by any tendency of the bar to go to the equatorial line; on the contrary, it will advance toward the axial, as if it were magnetic. It will not, however, continue its course until in that position, but, unlike any effect produced by magnetism, will stop short; and making no vibration beyond or about a given point, will remain there, coming at once to a dead rest: and this it will do even though the bar, by the effect of tension or momentum, was previously moving with a force that would have caused it to make several gyrations. This effect is in striking contrast

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with that which occurs when antimony, bismuth, heavy glass, or other such bodies are employed; and it is equally removed from an ordinary magnetic effect."

If pushed out of this dead position, it will return; although, before the electric current was circulating through the wire, it would swing round and round by the momentum and torsion: even when swinging, it could be caught and retained at the pleasure of the experimentalist. If, after this dead rest has been maintained for a second or two, the current is suddenly cut off, there is a sudden revulsion which causes the bar often to make two or three revolutions. If now the current be again laid on, the bar stops suddenly as before. When the bar is caught by the magnetic action in the equatorial or axial position, there is no revulsion on cutting off the current. In other cases, the shorter the continuance of the magnetic force, the less the revulsion. The bar produces similar effects if immersed in water, alcohol, mercury, &c.: and so likewise if inch plates of copper or bismuth intervene. With one magnetic pole, providing its face be large, the same general effects appear: if the face be small or conical they diminish, or even vanish altogether. These effects are best shown if the bar be suspended perpendicularly and opposite to the face of a single magnetic pole. In this case the magnetism fixes the bar, and the cessation of the power gives it a rotatory action. "The tendency, on making contact at the battery, is to place the longest moving dimension, *i. e.* the width of the bar, parallel to the line, joining the centre of action of the magnet and the bar."

"The bar, as before, is extremely sluggish, and as if immersed in a dense fluid, as respects rotation on its own axis; but this sluggishness does not affect the bar as a whole, for any pendulum vibration it has continues unaffected. It is very curious to see the bar jointly vibrating from its point of suspension and rotating on its axis when first affected by the magnetic force; for instantly the latter motion ceases, but the former goes on with undiminished power."

These effects seem due to the good conduction of copper for electric currents—to the *gradual* acquisition and loss of magnetism by the iron core—and hence to the production of induced currents of magneto-electricity. They occur only when the axis of rotation is perpendicular or oblique to the lines of magnetic force; and are at their maximum when the bar thus circumstanced is near to the magnet. These effects are fully explained by the action of the magneto-electric currents described in the second series of these researches; and the effect of which is to evolve a power in such a direction as

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shall tend to stop the motion. This sluggishness has been detected in other metals, which are given in the following order:—Copper, silver, gold, zinc, cadmium, tin, mercury, platinum, palladium, lead, antimony, bismuth. These phenomena accord with Arago's beautiful discovery, and with Herschel and Babbage's experiments. The author concludes by promising the results of his further researches on the action of magnets on magnetic metals, and on gases and vapours.

CLX. (III.) *On New Magnetic Actions, and on the Magnetic Condition of all Matter.* By Prof. FARADAY. (Continued.) Read before the Royal Society, Jan. 8, 1846.

(ABSTRACT.)

THE magnetic metals, which at certain high temperature cease to manifest magnetic action to the tests that have heretofore been applied to them, are found by this new test to retain some amount of magnetic power at all temperatures.

"A piece of iron wire, about one inch long and 0.05 of an inch in diameter, being thoroughly cleaned, was suspended at the middle by a fine platinum wire connected with the suspending thread, so as to swing between the poles of the electro-magnet. The heat of a spirit-lamp was applied to it, and it soon acquired a temperature which rendered it quite insensible to the presence of a good ordinary magnet, however closely it was approached to the heated iron. The temperature of the iron was then raised considerably higher by adjustment of the flame, and the electro-magnet thrown into action. Immediately the hot iron became magnetic, and pointed between the poles. The power was feeble, and in this respect the state of the iron was in striking contrast with that which it had when cold; but in character the force was precisely the same.

"The iron was then allowed to fall in temperature slowly, so that its assumption of the higher magnetic condition might be observed. The intensity of the force did not appear to increase until the temperature arrived near a certain point, and then, as the heat continued to diminish, the iron rapidly, but not instantaneously, acquired its high magnetic power; at which time it could not be kept from the magnet, but flew to it, bending the suspending wire, and trembling as it were with magnetic energy as it adhered by the end to the core."

Nickel, which at the temperature of boiling oil ceases to evince magnetic power in respect to ordinary tests, still pointed to and was attracted by the electro-magnet, however

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high its temperature, which was a good illustration of the retention of the power in magnetic metals. On lowering its temperature, the increase of power was gradual, and not instantaneous. Faraday had long since expressed an opinion that all metals might have the same character of magnetism, if their temperature could be sufficiently lowered; but the above facts are opposed to such a view; for the magnetic metals, under all circumstances, are magnetic metals, and the non-magnetic are non-magnetic—the one being attracted, and pointing axially—the other repelled, and pointing equatorially; so that he craves permission to withdraw the view he had put forth. The peroxides of iron, whether heated or otherwise, were found to be magnetic; indeed, they suffered *no* diminution of force by any heat applied. Oxide of nickel acted in a similar manner; so did oxide of cobalt. Glass coloured blue by cobalt was magnetic in a feeble degree. Every salt containing iron in the basic part was magnetic. The following are illustrations:—Protochloride, perchloride, iodide, protosulphate, persulphate, protophosphate, perphosphate, nitrate, carbonate, Prussian blue. Amongst native compounds:—Bog iron-ore, hæmatite, chromate of iron, yellow sulphuret of iron, arsenical pyrites, copper pyrites. And many others were magnetic.

Green glass and crown glass are magnetic from the iron they contain: flint glass is not, and it points equatorially. Crystals of yellow and red ferro-prussiate of potassa were not magnetic.

Solutions of the ferruginous salts were magnetic, and pointed axially; as, for instance, the proto- and per- sulphates and chlorides.

"These solutions apply a very important means of advancing magnetical investigation; for they present us with the power of making a magnet which is at the same time liquid, transparent, and, within certain limits, adjustible to any degree of strength. Hence the power of examining a magnet optically; hence also the capability of placing portions of matter one within another, and so observing dynamic and other phenomena within magnetic media. In fact, not only may these substances be placed as magnets in the magnetic field, but the field generally may be filled with them; and then other bodies and other magnets examined as to their joint or separate actions in it.

The sulphates and chlorides of nickel, as crystals and in solutions, were magnetic. All these solutions were still magnetic when heated. On the other hand, while these effects are obtained from compounds of the magnetic metals, "none

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of the compounds of the non-magnetic metals show as yet any indication of ordinary magnetic force." As an illustration of the power of iron in solution,—

"A clear solution of the protosulphate of iron was prepared, in which one ounce of the liquid contained seventy-four grains of the hydrated crystals; a second solution was prepared, containing one volume of the former and three volumes of water; a third solution was made of one volume of the stronger solution and fifteen volumes of water. These solutions I will distinguish as Nos. 1, 2, and 3; the proportions of crystals of sulphate of iron in them were respectively as 16, 4, and 1 per cent., nearly. These numbers may, therefore, be taken as representing (generally only) the strength of the magnetic part of the liquids. Tubes, like that before described, were prepared and filled respectively with these solutions, and then hermetically sealed—as little air as possible being left in them. Glasses of the solutions were also prepared large enough to allow the tubes to move freely in them, and yet of such size and shape as would permit of their being placed between the magnetic poles.

"In this manner the action of the magnetic forces upon the matter in the tubes could be examined and observed, both when the tubes were in diamagnetic media—as air, water, alcohol, &c.,—and also in magnetic media, either stronger or weaker in magnetic force than the substances in the tubes.

"When these tubes were suspended in air, between the poles, they all pointed axially or magnetically, as was to be expected—and with forces apparently proportionate to the strength of the solutions. When they were immersed in alcohol or water, they also pointed in the same direction; the strongest solution very well, and also the second; but the weakest solution was feeble in its action, though very distinct in its character."

When the tube of strongest solution was suspended in the strongest solution, it had no directive tendency: when in the medium, it pointed axially: when in the weakest, it took the same direction with greater power.

When the tube of medium solution was suspended in the strongest, it pointed *equatorially:* when in the medium solution, it was passive: when in the weakest, it pointed axially. The weakest solution in the weakest was inactive, but pointed equatorially in the other two. Similar results occurred with other ferruginous solutions. The tubes of solutions were now suspended vertically in solutions near single poles; and the general rule was, "that when the tube contained a stronger solution than that which surrounded it, it was attracted to the

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pole; but when its solution was the weaker of the two, it was repelled." It now occurred, that as the compounds of magnetic metals are magnetic, the compounds of other metals—of whose magnetic character doubts were entertained—might serve to determine their nature.

"*Titanium.*—Wollaston has described the magnetic effects of crystals of titanium, expressing at the same time a belief that they are due to iron. I took a specimen of the oxide of titanium, which I believe to be perfectly free from iron, and, enclosing it in a tube, subjected it to the action of the electromagnet. It proved to be freely magnetic.

"Another specimen, obtained from Mr. Johnson, and believed by him to be perfectly free from iron, was also magnetic. Hence I conclude that titanium is truly a magnetic metal."

Manganese.—Berthier had announced manganese to be a magnetic metal; and his opinion appears correct; for the oxides, the chloride, sulphate, &c., in crystals; and the chloride, nitrate, sulphate, &c., in solution, were magnetic. The author thinks, therefore, that manganese at a low temperature possesses considerable magnetic power.

Cerium.—Its hydrated protoxide, carbonate, chloride, were all magnetic; and so were the solutions of the soluble salts.

Chromium.—The crystallized oxide was magnetic; chromic acid feebly so. Chromate of lead pointed equatorially, and was repelled;—as did also crystals of chromate of potassa. Crystals of the bi-chromate were, if any thing, magnetic. But "solutions of either salt pointed well equatorially, and were repelled; thus showing the diamagnetic influence of the water present." Solution of the bi-chromate in a tube pointed equatorially; but, if a little alcohol and some muriatic or sulphuric acid were added, and heat applied so as to reduce the chromic acid to a state of oxide or chloride, it then becomes strongly magnetic: so that there is now no doubt about chromium being a magnetic metal.

Lead.—The chloride, iodide, sulphuret, nitrate, phosphate, carbonate, fused protoxide, and acetate of lead pointed equatorially, and were repelled. Crystallized nitrate was dissolved and precipitated by pure zinc, and the pure lead thus obtained; and was free from magnetism—ranking, therefore, as a diamagnetic, and not, as it has usually seemed to be, as a magnetic metal.

Platinum.—Mr. Faraday has found no wrought specimens free from magnetism, not even those prepared by Dr. Wollaston, and presented to the Royal Society. Those obtained from Mr. Johnson were all slightly magnetic. The dry

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chloride and its solution pointed equatorially and were repelled; as did also the ammonio-chloride. A portion of this ammonia-chloride was heated, and spongy platinum produced: this pointed *axially*, and was attracted. So that platinum is a metal slightly magnetic; and, when in combination, it acquires diamagnetic characters from the presence of other bodies.

Palladium.—All the palladium in the possession of the Royal Society, prepared by Dr. Wollaston is magnetic; as, to a slight degree, were all Mr. Johnson's specimens. The chloride, ammonia-chloride, and cyanuret were repelled, and pointed equatorially. The metal obtained by reducing the cyanuret was feebly magnetic. The metal precipitated from solution of nitro-muriate by pure zinc was, in *all* cases, magnetic. Ammonio-chloride was diamagnetic; but the metal obtained from it was magnetic. So that palladium is to be regarded as a magnetic metal.

Arsenic.—This metal presented appearances at times favouring the magnetic, and at other times the diamagnetic: on the whole, it appears to belong to the latter, and but little removed from zero-point.

Osmium.—Crystals of osmic acid were diamagnetic; the metal and the prototoxide were slightly magnetic. Probably, therefore, osmium belongs to the magnetic class.

Iridium.—The oxide, chloride, and ammonia-chloride were magnetic; so was a sample of the metal. One sample which seemed pure was scarcely at all magnetic. "On the whole," says the author, "I incline to believe that iridium does not stand in the magnetic class."

Rhodium.—A well-fused specimen, prepared by Dr. Wollaston, was magnetic; but crystals of the chloride and sodio-chloride were not. The metal is inferred to be diamagnetic; or, if magnetic, only just removed from zero point.

Uranium.—The peroxide was not magnetic; the protoxide very slightly so. The metal is for the present placed in the diamagnetic class.

Tungsten.—The oxide and the acid pointed equatorially: the acid was repelled by a single pole; the oxide remained nearly neutral. For the present it is considered a diamagnetic.

Silver, antimony, bismuth.—Neither the metals nor their compounds are magnetic.

Sodium.—A large globule, equal to half a cubic inch was repelled; and is, therefore, diamagnetic.

Magnesium, calcium, strontium, barium, sodium, potassium, ammonia.—None of the compounds or salts are magnetic.

It would, therefore, appear that besides iron, nickel, and

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cobalt, the following are magnetic metals:—titanium, manganese, cerium, chromium, palladium, rhodium. There may be others presenting distinct magnetic power, but in a very slight degree.

The author is of opinion that by cooling some of the magnetic metals, they may evince the power in a higher degree.

The following is an attempt to arrange some of the metals in order as respects their relation to magnetic force. The 0° or medium point is supposed to be the condition of a metal or substance indifferent to the magnetic force as respects attraction or repulsion in air or space. The further substances are placed from this point, the more distinctive are they as regards their attraction or repulsion by the magnet. Nevertheless this order may, very probably, be found inaccurate by more careful observation.

MAGNETIC.	DIAMAGNETIC.
Iron.	Bismuth.
Nickel.	Antimony.
Cobalt.	Zinc.
Manganese.	Tin.
Chromium.	Cadmium.
Cerium.	Sodium.
Titanium.	Mercury.
Palladium.	Lead.
Platinum.	Silver.
Osmium.	Copper.
	Gold.
	Arsenic.
	Uranium.
	Rhodium.
	Iridium.
	Tungsten.

The apparently middle condition between magnetics and diamagnetics, which air occupied, led the author to many experiments upon its probable influence. A thin tube of flint-glass—in the one case full of air; and in other cases with rarified air; and finally with as good a vacuum as could be obtained—was placed between the magnetic poles; and, in each case, it evinced a similar slight equatorial tendency, due evidently to the glass. Suspended under various conditions in hydrogen or in carbonic acid, it exhibited the same indifference. "Hence there appears no sensible distinction between dense or rare air; and, as far as those experiments go, between one gas or vapour and another." Thinking that the attractions and repulsions might depend upon the converse action of the

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media with which bodies were surrounded, Professor Faraday proceeded to examine the behaviour of diamagnetic substances, when the air or gas was altered in density, and also of air itself when surrounded by these media. The air-tube was suspended in water, being kept beneath the surface by a piece of bismuth: when subjected to the magnetic forces it pointed axially, as a magnet would have done. In alcohol or in oil of turpentine, the same results obtained as in water; and so also in mercury. A bar of heavy glass, or a bar of bismuth, was now suspended in air; it pointed equatorially, and apparently with the same degree of force, whether the air were more or less rarified. Carbonic acid and hydrogen were successively introduced, without producing any change. A bismuth tube was suspended in air, and gases at ordinary pressure, or highly rarefied; but the repulsion from a single pole was the same. A copper bar acted in vacuo just as it did under ordinary circumstances. Tubes of vacuum, air, hydrogen, carbonic acid gas, sulphurous acid gas, vapour of ether were successively suspended in water, and then in alcohol: they all pointed axially, and apparently with equal force. The same preparation, when surrounded by air or by carbonic acid gas, all set equatorially. The axial position in these experiments is undoubtedly due to the relation between the contents of the tube and the surrounding medium; for any effect of the tube itself would be to give them an equatorial set. But when the tubes were in surrounding gases the equatorial direction is due to this constant feeble force; as for instance,—

I suspended a tube of liquid sulphurous acid in gaseous sulphurous acid: when, under the magnetic influence, the liquid pointed well equatorially. I surrounded nitrous liquid acid by gaseous nitrous acid: the liquid pointed well equatorially. I placed liquid ether in the vapour of ether: the former pointed equatorially. Upon suspending the tube of vapour of ether in liquid ether, the vapour pointed axially.

All gases and vapours seem to occupy a middle place between magnetics and diamagnetics; and are alike in their magnetic relation, and all equal to a perfect vacuum, whatever be their density or chemical character. "Bodies which are very marked as diamagnetic substances, immediately lose all trace of this character when they become vaporous. It would be exceedingly interesting to know whether a body from the magnetic class, as chloride of iron, would undergo the same change."

These are the general facts; and the equatorial tendency

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of an elongated portion of matter may be resolved into a mere repulsion from either pole; and "all the phenomena resolve themselves into this,—that a portion of such matter, when under magnetic action, tends to move from stronger to weaker places or points of force. When the substance is surrounded by lines of magnetic force of equal power on all sides, it does not tend to move, and is then in marked contradistinction with a lesser current of electricity under the same circumstances."

This condition and effect is new, not *only* as respects the exertion of power by a magnet over bodies previously supposed to be indifferent to its influence, but is *new* as a magnetic action, presenting us with a second mode to which the magnetic power can exert its influence.

"These two modes are in the same general arithmetical relation to each other as positive and negative in electricity, or as northness and southness in polarity, or as the lines of electric and magnetic force in magneto-electricity. And the diamagnetic phenomena are the more important, because they extend largely, and in a new direction, that character of duality which the magnetic force already, in a certain degree, was known to possess.

"So all matter appears subject to the magnetic force as much as it is to the gravitating, and substances are arranged into two distinct classes, between which the contrast is so great, that when one kind is attracted the other is repelled; and when a bar of one assumes a certain position, a bar of the other places itself at right angles. The author has not yet found a solid or a fluid (not being a mixture) that is perfectly neutral—it would be important to know if there really were such a body: of compound or mixed bodies there are many; and, as such bodies are required in investigations, the preparation is described.

"Protosulphate of iron belongs to the magnetic, and water to the diamagnetic class; and, using these substances, I found it easy to make a solution which was neither attracted nor repelled, nor pointed when in air. Such a solution pointed axially when surrounded by water. If made somewhat weaker in respect of the iron, it would point axially in water, but equatorially in air; and it could be made to pass more and more into the magnetic or the diamagnetic class by the addition of more sulphate of iron, or more water.

"Thus a fluid medium was obtained which practically, as far as I could perceive, had every magnetic character and effect of a gas, and even of a vacuum; and, as we possess both magnetic and diamagnetic glass, it is evidently possible to prepare

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a 'solid' substance possessing the same neutral magnetic character."

As a fluid neutral medium is thus obtained, and as there is a magnetic and diamagnetic glass, it is evidently possible to obtain artificially a solid neutral body. The following list has been drawn out, as a general sketch of the relative position of bodies:—

Iron . . .	Nickel
Cobalt . . .	Manganese
Palladium . .	Crown glass
Platinum . .	Osmium
0° Air and vacuum .	Arsenic
Ether . . .	Alcohol
Gold . . .	Water
Mercury . . .	Flint glass
Tin . . .	Heavy glass
Antimony . .	Phosphorus
	Bismuth.

It is curious to find metals at the extremities of the list; and also that those at each extreme, iron and bismuth, are bad conductors of electricity. "At the same time, the *contrast* between these metals, as to their fibrous and granular state, their malleable and brittle character, will press upon the mind whilst contemplating the possible condition of their molecules when subjected to magnetic force."

With respect to the metals, we not only learn that they are not all magnetic as iron, but that some of them are precisely the reverse. It is scarcely possible not to refer the attraction on the one hand, and the repulsion on the other, to action on the molecules of matter, by which they are thrown into different conditions and affected accordingly.

In that point of view it is very striking to compare the results with those that are presented to us by a polarized ray, especially as then a remarkable difference comes into view; for if transparent bodies be taken from the two classes, as, for instance, heavy glass or water from the diamagnetic, and a piece of green glass or a solution of green vitriol from the magnetic class, then a given line of magnetic force will cause the repulsion of one and the attraction of the other; but this same line of force, which thus affects the particles so differently, affects the polarized ray when passing through them precisely in the "same" manner in both cases; for the two bodies cause its rotation in the "same" direction.

These conditions are the more important when connected with the diamagnetic and the optical properties of the polarized ray. Thus iron solution and a piece of quartz,

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each having power to rotate a ray, point, by the influence of the *same* magnetic force, one axially, the other equatorially; but the rotation impressed by each of these bodies on a ray of light, under the influence of the magnetic force, is the same. There is one essential difference in this rotative power, "for quartz by itself can only rotate the ray in one direction, but under the influence of the magnetic force it can rotate it to the right and the left, according to the course of the ray." Or, if two pieces of quartz, or two tubes of oil of turpentine, rotate a ray in different ways, any further rotative power given to them by magnetism is always in the same direction. The conditions arising from these contrasts will be the subject of further investigation before the author submits them to the Royal Society.

The theory of the movement of diamagnetic bodies might be explained on the assumption that magnetic induction produced in them a contrary state to that produced in magnetic matter—*i. e.*, that each would become magnetic, but while magnetic matter would have its poles facing poles of the contrary name, diamagnetic matter would have these facing poles of the same name. Upon Amperè's theory this would be equivalent to saying that as currents are induced in magnetics parallel to the inducing current or magnet, so in bismuth, &c. the currents are in the contrary direction. In respect to non-conducting bodies of either class, no difficulty would occur, as the currents are presumed to exist not in the mass, but round the particles of matter. As far as experiment has gone, the inductive effects on *masses* of either are the *same*. If a rod of iron on the one hand, or a rod of bismuth on the other, be carried across magnetic lines, electric currents arise in the *same* determinate direction: in this case the induction is manifested in the masses, whilst in the cases now under consideration "the currents, if they exist, are probably about the particles of the matter."

The magnetic relation of æriform bodies is exceedingly remarkable.

That oxygen or nitrogen gas should stand in a position intermediate between the magnetic and diamagnetic classes; that it should occupy the place that *no* solid or liquid element can take; that it should show no change in its relations by rarefaction to any possible degree, or even, when the space it occupies passes into a vacuum, that it should be the same magnetically with any other gas or vapour; that it should not take its place at one end, but in the very middle of a great series of bodies; and that all gases or vapours should be alike, from the rarest state of hydrogen to the

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densest state of carbonic acid, sulphurous acid, or ether vapour, are points so striking as to persuade one at once that air must have a great and perhaps an active part to play in the physical and terrestrial arrangement of magnetic forces.

The author at one time thought that, as air and gases admitted of alternation, he had a means at hand for examining corresponding variations in their magnetic character; but these hopes are now taken away. The disappearance of magnetic condition, when bodies pass into the gaseous state, is remarkable; for no gas or vapour has yet been shown to have any action upon the polarized ray. Whether the negative action of gases and vapours is due to the small quantity of matter they contain, or to the altered physical condition, is a point of great importance for the theory of magnetism. The author's idea for carrying out the experiment is with one of Caigniard de la Tour's ether tubes; but he expects a difficulty on account of the mass (for strength's sake) of the tube that will be necessary.

It is very striking to observe the feeble condition of a helix when alone, and the astonishing force which, in giving and receiving, it manifests by association with a piece of soft iron. So also here we may hope for some analogous development of this element of power, so new as yet to our experience. It cannot for a moment be supposed that, being given to natural bodies, it is either superfluous, or insufficient, or unnecessary. It doubtless has its appointed office, and that one which relates to the whole mass of the globe; and it is probably because of its relation to the whole earth, that its amount is necessarily so small (so to speak) in the portions of matter which we handle and subject to experiment. And small as it is, how vastly greater is this force, even in dynamic results, than the mighty power of gravitation, for instance, which binds the whole universe together, when manifested by masses of matter of equal magnitude!

He considers this explained by the assumption that bismuth and its congeries are absolutely repelled, when nothing but themselves are concerned; and iron and its similars are attracted; and they explain the axial and equatorial pointing of air by considering it "as due to the travelling of the air to those positions which the magnetic or diamagnetic bodies tended to leave." The effects of air in these results are precisely similar to what was obtained in solutions of iron of different strengths, where the effects were due to the greater or lesser degree of magnetic power possessed by the solutions. "It is easy to perceive that if all bodies were magnetic in

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different degrees, forming one great series from end to end, with air in the middle of the series, the effects would take place as they actually occur." Any body from the middle part of the series would point equatorially to the bodies above it, and axially to those below; for a body, as bismuth, which goes from a strong to a weak point of action, may do so only because the substance then at the weak place may desire to come to the stronger place. "And so air in water, or even under mercury, is, or appears to be, drawn towards the magnetic pole." But if this were the true view, then it might be expected that rarefaction of the air would make it more diamagnetic, or at least alter its place in the list; but this is not the case. And such a view would make mere space as much magnetic as air and the gases; now though "space, air, and the gases have the same general relation to the magnetic force, it seems to me," says Faraday, "a great additional assumption to suppose that they are all absolutely magnetic, and in the midst of a series of bodies, rather than to suppose that they are in a normal or zero state." And for the present, he thinks that diamagnetics have an action distinct from ordinary magnetic action, and that they thus present "a magnetic property new to our knowledge." The amount of this power measured by its dynamic effect is small; but it is probable that the time may come when other indicators or measurers may be found.

Fully assured that the uses of this power in nature will be developed hereafter, the author ventures a few hasty observations. Matter itself must evidently be concerned in these reactions; as iron, when under magnetic influence, exercises its newly-acquired power over other bodies. Faraday adds:—

"In former papers I proposed a theory of electrical induction founded on the action of contiguous particles, with which I am now even more content than at the time of its proposition; and I then ventured to suggest that probably the lateral action of electrical currents, which is equivalent to electrodynamic or *magnetic* action, was also conveyed onward in a similar manner. At that time I could discover no peculiar condition of the intervening or diamagnetic matter; but now that we are able to distinguish such an action, so *like* in its nature in bodies so *unlike* in theirs, and by that so like in character to the manner in which the magnetic force pervades all kinds of bodies, being at the same time as universal in its presence as it is in its action; now that diamagnetics are shown not to be indifferent bodies, I feel still more confidence in repeating the same suggestion, and asking whether it may

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not be by the action of the contiguous or next succeeding particles that the magnetic force is carried onwards, and whether the peculiar condition acquired by diamagnetics when subject to magnetic action, is not that condition by which such propagation of the force is affected?"

Whether bodies are divided into two lists, or occupy one great magnetic class, the general question is not affected; for it is the differential relation of any two substances from different places in the list that governs their mutual effects. The only place of difficulty is in air, vapours, and gases; but here such wonderful physical changes occur that we might almost expect some peculiar condition to be assumed. Electric induction is varied enough among solid and liquid bodies, but in air or gases the amount is alike in all, and also whether they be dense or rare; and there would be no cause for surprise if magnetic action were found to correspond in this respect. In reference to electric induction across a vacuum, the author has nothing to add to what he has formerly expressed. In thinking of the crust of the earth, composed as it is of an enormous mass of diamagnetics, acted on by magnetic curves, we cannot doubt but that some great purpose of utility is designed. Diamagnetics form by far the greater part of the crust of the earth; and though proportionately less powerful in their own action than the magnetic bodies are in theirs, we must not hastily assume that they are entirely overruled. The ocean, seas, &c., are diamagnetic without other influences, and of rocks and mountains the diamagnetic influence is greater than might have been anticipated.

"I mentioned that by adjusting water and a salt of iron together I obtained a solution inactive in air; that is, by a due association of the forces of a body from each class, water and salt of iron, the magnetic force of the latter was entirely counteracted by the diamagnetic force of the former, and the mixture was neither attracted nor repelled. To produce this effect it required that more than 48·6 grains of crystallized protosulphate of iron should be added to ten cubic inches of water (for these proportions gave a solution which set equatorially), a quantity so large that I was greatly astonished on observing the power of the water to overcome it. It is not, therefore, at all unlikely that many of the masses which form the crust of this our globe may have an excess of diamagnetic power, and act accordingly."

Though the earth's magnetic curves are divergent in their general forms, yet the magnitude of the system prevents our

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observing places of stronger or of weaker power, within small limits, that any attempt to observe the passage of matter from a stronger to a weaker place would fail.

"Theoretically, however, and at first sight, I think that a pound of bismuth, or of water, estimated at the equator, where the magnetic needle does not dip, ought to weigh less when taken into latitudes where the dip is considerable; whilst a pound of iron, nickel, or cobalt, ought, under the same change of circumstances, to weigh more. If such should really prove to be the case, then a ball of iron and another of bismuth, attached to the ends of a delicate balance beam, should cause that beam to take different inclinations on different parts of the surface of the earth; and it does not seem quite impossible that an instrument to measure one of the conditions of terrestrial magnetic force might be constructed on such a principle."

Speculating upon the whole system of curves or very large masses in plates or rings, we might expect them to place themselves equatorially; and on this principle the ring of Saturn would place itself where it actually is. It is curious to see wood, beef, apple, &c., repelled; or a leaf to place itself equatorially: but whether such directive actions are manifested in nature remains to be proved.

Of the interior of the earth, we have reason to think it of a high temperature; and hence, the author had recently remarked, at a certain distance beneath the surface, magnetic substances must cease to evince magnetic properties. But he now finds that he was in error; for he has in these last researches proved that magnetic metals retain a certain degree of their properties, however high their temperature may be raised. So that though the deep contents may not form magnets of themselves, "they are just in a condition to act as a soft iron core to the currents around them." The author concludes with the following observations:—

"If the sun have anything to do with the magnetism of the globe, then it is probable that part of its effect is due to the action of the light that comes to us from it; and in that expectation the air seems most strikingly placed round our sphere, investing it with a transparent diamagnetic, which, therefore, is permeable to its rays, and at the same time moving with great velocity across them. Such conditions seem to suggest the possibility of magnetism being there generated; but I shall do better to refrain from giving expression to these vague thoughts (though they will press in upon the mind), and first submitting them to rigid investiga-

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tion by experiment, if they prove worthy, then present them hereafter to the Royal Society."

Dated Dec. 22, 1845.

In a subsequent note, dated Feb. 2, 1846, the author gives the following notes and references :—

Brugnons first observed the repulsion of bismuth by a magnet in 1778. *Antonii Brugnons Magnetismus seu de affinitatibus Magneticiis observationes Magneticæ.* Ludg. Batav. 1778, sec. 41.

M. Le Baillif on the Repulsion of a Magnet by Bismuth and Antimony; Bulletin Universal, 1827, vol. vii. p. 371; vol. viii. pp. 87, 91, 94.

Saijey on the Magnetism of certain Natural Combinations of Iron; and on the Mutual Repulsions of Bodies in General. *Ibid.* 1828, vol. ix. pp. 89, 167, 239.

Seebeck on the Magnetic Polarity of different Metals, Alloys, and Oxides. *Ibid.* 1828, vol. ix. p. 175.

Note.—In drawing out the above abstract, we have been very careful to keep the spirit of the author before us; and to express *his* facts and *his* inferences throughout. The contents of this abstract must be regarded as being essentially *his*, expressed in brief.—C. V. W.

CLXI. *Can Magnetism exercise any Influence over the Circulation of the* Chara? By M. Dutrochet. "Comptes Rendus," April 13, 1846. Vol. xxii. p. 619. No. 15.

(Translation.)

Since we have become possessed of the powerful electro-magnets produced by the action of the electricity of the pile upon soft iron, we have had the opportunity of making experiments on magnetic action that are much more conclusive, and much more interesting, than it was possible to make with the natural or artificial magnets that we had the opportunity of using; for they are so much more feeble. At present, we can possess at pleasure magnetic forces so enormous that these are, so to speak, beyond the possibility of being measured. I here undertake to subject stalks of the Chara to these powerful electro-magnets, in order to see if the circulation that exists in these tubular stalks would experience any influence on their part. I had already found, by experiment, that a magnetized steel bar, when brought near to these stalks, did not exercise any influence over their circulation. M. Pouillet was kind enough to place at my disposal the powerful electro-

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magnet that he employed in repeating Faraday's experiments, which were communicated to the Academy at the meeting of Jan. 26.* The following is the description given by him of one of these electro-magnets :—

"They can support as much as 800 k.g. (1764½ lbs.) when they are magnetized by a battery of 20 pairs. They are cylinders of soft iron, 7 or 8 c.m. (3 in.) in diameter, and about 50 c.m. in length, which are bent horse-shoe fashion : the distance of the axis, or two branches or poles, being only 12 or 15 c.m. (4½ or 5 in.) There were 500 or 600 metres of copper wire rolled round each branch."

Before commencing the exposition of the experiments that I made with this electro-magnet upon the circulation of the *Chara*, I must briefly call to mind those that I made in 1837, in concert with M. Becquérel, upon the influence exercised by electricity over this same circulation.

When a twig, or rather a merithal of Chara, is subjected to a very feeble electric current, which traverses this merithal in the direction of its length, the circulation continues in this merithal until it is subjected to the action of a current sufficiently powerful to arrest it; which is accomplished by successively increasing the number of the elements of the pile. In this case the cessation of the circulation lasts only for a few minutes, and the electric current preserving the intensity which had brought about the cessation of the circulation, the latter begins again, or re-establishes itself spontaneously. A fresh increase in the number of the pairs of the pile, namely, a new increase in the intensity of the electric current, determines a new suspension in the circulation of the Chara—a circulation which is spontaneously re-established a few minutes afterwards, notwithstanding the continuity of the intensity of the electric current that had occasioned its suspension. The same phenomena of the suspension of the circulation, and of its spontaneous re-establishment, are equally observed by submitting the Chara to electric currents whose intensity is sufficiently and suddenly diminished; so that it is enough that if the electric current undergoes a sufficient change in intensity, either in regard to increase or to diminution, for the circulation of the Chara submitted to these electrical experiments to be suspended for a certain time; after which it is spontaneously re-established, notwithstanding the continuous influence of the electric current that had occasioned its suspension.

This influence of electricity on the circulation of the Chara is precisely similar to that which all *exciting* causes exercise

* Vide *Elec. Mag.* No. 11. p. 262.

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over this same circulation. Thus on transferring a twig of Chara from water, whose temperature is 7° C. (45° Fahr.), to water heated to 32° C. (90° Fahr.), the circulation ceases after about five minutes; and the same heat of 32° C., which had occasioned the cessation of the circulation, continuing to exist in the water surrounding the Chara, this circulation is spontaneously re-established in this plant after an hour of suspension. When this circulation was well re-established, I replaced the Chara in water whose temperature was 7° C.; I again saw the circulation cease after four minutes, remain thus suspended for an hour and a half, and spontaneously re-establish itself after this time had passed away. Another example:—A twig of Chara having been taken out of soft water and transferred into water of the same temperature which held in solution $\frac{1}{90}$th of its weight of sea-salt, the circulation ceased after four minutes, and was re-established after eight minutes' suspension. After having remained for ten hours in this salt water, in which it preserved its circulation, this twig of Chara was replaced in soft water of the same temperature: the circulation stopped at the end of four minutes, and did not begin again until after a suspension of five minutes.

These observations, to which I have joined many others of an analogous kind,* have proved that, under the influence of causes termed *exciting*, the circulation of the Chara diminishes in velocity, or altogether ceases, and is re-established with a velocity often superior to its initial velocity, after a suspension of a certain duration; and this by a veritable re-action of the vital force against the exterior force, which tends to abolish it, and to the action of which it resists in endeavouring to place itself in *equilibrium* with it. I have shown that even when this inimical force is too energetic, the vital force still resists for a certain time; and does not cease to exist until its effort of re-action has been overcome. Now, since observation shows us that the electric current produces the same phenomena—that its augmentation, as well as its diminution, of intensity equally produce the cessation of the circulation of the Chara,—a circulation which is then established spontaneously, notwithstanding the continuity of the electric action which had occasioned this cessation,—this proves to us that electricity acts upon the Chara in the same manner as the other causes termed *exciting:* whence it follows that the vital force which brings about the circulation, and the

* Vide my Memoir inserted in the eighteenth volume of *Mémoires de l'Académie des Sciences*, p. 439; and in the *Annales des Sciences Naturelles*. 2nd Series, Vol. ix.

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electric force, are two different forces. Let us now see whether the same is the case with the magnetic force, which is but a particular modification of the electric force.

As was mentioned above, we had at our disposal a magnetic force capable of sustaining a weight of 800 k.g., when the electro-magnet was excited by 20 pairs of a Bunsen's pile,—we excited this same electro-magnet with 50 pairs, and we thus had a magnetic force which was not measured, but which I think may, without error, be estimated at 2000 k.g. I subjected to this prodigious magnetic force a twig of *chara vulgaris*, which I placed horizontally, and in the direction of its length between the two poles of the electro-magnet, bent horse-shoe fashion, the poles being placed in the same horizontal plane. The twig was 2 c.m. in front of the vertical plane that passed through its poles, which caused very little diminution in the action of the magnetic force upon it; for M. Pouillet has found by experiment that at the distance of 10 c.m., in the same direction, the magnetic action *is still a considerable portion of what it was during actual contact.** The circulation without difficulty by means of the microscope in the twig of Chara that is subjected to experiment. I was careful to observe what might happen at the moment of the establishment of the electric current, by which the magnetic force was generated: I observed no change occur in the velocity of the circulation. I left this twig of Chara subjected to this enormous magnetic force for 10 minutes: the circulation was not influenced. I suddenly changed the two poles of the electro-magnet, by means of a commutator applied to the conducting wires of the electric current: the circulation did not undergo any change. I subjected the twig to the influences of one pole only, by making each of them correspond alternately, sometimes above, sometimes below the twig: I did not obtain any effect upon the circulation. After each of these experiments, I entirely suppressed the magnetic influence: no change of velocity was manifested to the circulation. Thus was it demonstrated to me that the magnetic force, even when it is prodigious, exercises no influence over the circulation of the Chara. There exists, therefore, no relation between the vital force that produces this circulation and the magnetic force.

It follows from all this that the vital force, by which the circulation of the Chara is brought about, is not the electric force, since the latter acts upon this circulation like any other *exciting* causes, and that this vital force has no relation to the

* *Comptes Rendus*, vol. xxii. p. 142.

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magnetic force, since the latter is deprived of all influence over the said circulation.

Thus, we must recognise that the vital force is a force *sui generis*,—of the nature, the relations, and mechanism of which we possess no notion.

These observations must of necessity change the opinions of those who have regarded the *vital force* as an *imaginary being*.

We must recognise at the same time that all the causes termed *exciting* are *debilitating* and *sedative* by their *primitive* and *direct* effect; and that they are *fortifying*, *stimulant*, and *tonic* by their *secondary* or *indirect* effect; and this by the effect of the *vital reactions* which they occasion either instantaneously, or with some delay.—[A.]

SCIENTIFIC MEETINGS.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

A PAPER entitled "An Experimental Investigation of the Characters of Simple Metals, Metallic Alloys, and Metallic Salts" was read at this society on April 7th and May 5th by Mr. William Sturgeon, late of the Victoria Gallery, Manchester. After having taken a retrospective view of the labours of other experimentalists in this department of magnetics, the author proceeded to describe a novel apparatus, by means of which his own experiments had been made. It consists principally of a powerful magnet and a light wooden lever, delicately suspended by a few silken fibres, the whole being enclosed in a glass case. By means of this apparatus Mr. S. has found that gold, silver, copper, platinum, antimony, bismuth, lead, tin, and some other metals, when pure are perfectly neutral to the action of the magnet; but when some of them are combined they become magnetic in an eminent degree. Some of his earliest experiments (especially those on brass), many specimens of which he found to be magnetic, led him to suppose that their magnetic actions were due to small portions of iron which they contained; for in some of the metals of commerce on which he experimented, iron was absolutely present. In a mass of bismuth, for instance, iron was found in considerable proportions. Mr. S. had discovered that all our silver coinage is magnetic, but in different degrees, according to the dates of its production. Domestic articles of silver, as spoons, &c., are still more magnetic than silver coins. Gold coins are also magnetic, but not so highly as silver ones. The gold of articles of jewellery is more magnetic than gold coins. Mr. S. was still of opinion that these alloys might possibly contain iron, and their magnetism be therefore dependent on that metal, although he had met with some facts which he could not

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reconcile to that view. Having found much more magnetism in an old half-crown piece of William and Mary than in any other coin, and the historical report being, that the silver coinage of that period was in a very base state, he supposed it possible that the high magnetic action of the old half crown might be due to the extra proportion of copper which led him to form an alloy of pure silver and pure copper in which the latter metal formed about one-sixth part of the whole; and what is very remarkable, this alloy was more highly magnetic than any silver article he had previously examined. The copper and silver of this alloy had previously been ascertained to be non-magnetic; they were melted in an earthen crucible, and no iron entered the alloy. This fact and some others which he subsequently discovered, shook the opinion Mr. Sturgeon had first formed respecting the presence of iron in brass, and the other alloys which showed magnetic action. Cavallo had discovered the magnetism of brass, and showed that hammered brass was never more magnetic than when unhammered, which facts corresponded to Mr. S.'s own observations. But the most capital discovery had yet to be made. Mr. S. formed an alloy of iron and zinc in equal parts, and, to his surprise, found that this alloy was almost neutral to the magnet; so much so that it would not move a magnetic needle at half an inch from the pole.

In another alloy of these metals, the iron being about one-eighth of the zinc, magnetism was quite extinct. These novel facts naturally led to the inference that, as zinc is an effectual constituent of brass, the magnetism of that alloy could not be owing to its containing iron; and subsequent experiments, both magnetic and chemical, have shown that this is the fact.

Antimony has long been known to deteriorate the magnetism of iron; but Mr. S. has ascertained that its neutralizing powers are much inferior to those of zinc. An alloy of antimony and iron, in which the latter metal was less than a twentieth part, still showed magnetic action, though in a slight degree. Mr. S. has also discovered that nickel loses all its magnetic powers when alloyed with about ten times its weight of zinc. Antimony also counteracts the magnetism of nickel, but not so powerfully as zinc. German silver, which contains a large proportion of nickel, is slightly magnetic; but in the inferior kinds of this alloy, where nickel is less abundant, no magnetism is perceptible. The neutralising metal in these cases is zinc.

The salts of iron which Mr. S. has examined are mostly magnetic; but what is remarkable, their magnetism bears no proportion to the proportions of iron they contain. In conforming his theoretical views to the facts thus developed, Mr. S. now considers all bodies to be more or less magnetic, and classes them into two grand divisions—those that are palpably magnetic, and those that are but obscurely magnetic. The former class he calls *sapho-magnetics*, and the latter *asapho-magnetics*.

Sapho-magnetics are again divided into *mono-magnetics*, and

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suno-magnetics, according as they consist of individual or compound bodies respectively.

Those bodies which counteract the magnetism of others Mr. S. calls *katalo-magnetics*, because many of them (if not all) have the power of completely neutralising even the highest powers of *mono-magnetic* bodies.

The *mono-magnetics* at present known are iron and nickel, and perhaps cobalt. In the class *suno-magnetics* Mr. S. places alloys of silver and copper, gold and copper, and zinc and copper, under the expectation that many more will soon be added to their number.

The *katalo-magnetics* are very numerous, being all those that deteriorate the magnetism of other bodies.

Amongst the metallic *katalo-magnetics* Mr. S. has found zinc, antimony, tin, and lead. Other *katalo-magnetics* are sulphur, oxygen, chlorine, cyanogen, and the generality of those bodies which combine with metals.—From *Literary Gazette*, June 6th, 1846.

ROYAL ACADEMY OF SCIENCES, PARIS. 1846.

(*Continued from page* 315.)

Feb. 9.—*Examination of the troubled Part of the Liquid Vein.* By M. Ch. Matteucci. Letter to M. Arago.

"I think that it might be useful in certain public demonstrations to make known a very simple experiment that I lately made in my course of lectures, in reference to Savart's beautiful observations on the liquid vein. It is well known that Savart succeeded in explaining the construction of the troubled part of the vein, in which the belly-shaped appearances are manifested, by admitting that this was due to drops, which assumed successively different forms, namely, which contracted and lengthened alternately.—Savart's hypothesis is a fact very easily verified: we have merely to illuminate the vein with a large electric spark, or, what is better, with a series of sparks. By examining the vein thus, just as Savart obtained it, any one may easily see that the part which seems continuous to the eye, is composed of drops having exactly the form given to them by Savart, the so elongated drops, and others that are flattened, among which are other drops almost spherical or more flattened. I think that, with a similar arrangement adopted in a magic lantern, we might project the vein on a larger screen, and yet illuminate it with the spark, in order to see the drop."

Observations on a Change produced in the Elasticity of a Bar of soft Iron, under the influence of Electricity. By M. Guillemin.

"I have the honour to communicate to you the result of an experiment, which appears to demonstrate that the elastic force of a bar of soft iron is increased by magnetization.

"To prove this fact, we have merely to place horizontally a bar

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of iron covered with a helix of copper wire, and sufficiently long to be flexible, fixing it by one end, the other end being free; in order that the effect may be more sensible, the latter extremity is loaded with a slight weight.

"The apparatus being thus arranged, every time that a current is passed in the wire, the bar rises, and raises this, which it holds suspended as long as the current passes, and allows it to fall again when the circuit is interrupted.

"This action is not very energetic, but it is sufficiently sensible to be proved without any micrometric apparatus, by using a single Bunsen's element, and a bar 1 c.m. in diameter, by 20 or 30 long.

Magnetism.—Extract from a letter from M. De Haldat, to M. Arago.

"In a glass tube 8 c.m. (3 in.) long, let there be placed a great number of very small hard steel magnets half a m.m. ($\frac{1}{50}$ of an inch) in diameter, or 3, 4, or 5 m.m. ($\frac{1}{10}$ to $\frac{1}{5}$ of an inch) in length, assembling them irregularly, so that a great number cross each other in different directions, such a magnet constantly takes the direction transverse to the magnetic current, when suspended to a silk thread, composed of four simple filaments.

"In this experiment must we not see the effects of the molecules of bodies magnetically polarized? From this moment, I might, with reference to this subject, give myself over to a host of conjectures; but I reserve myself, that I may return to them upon another opportunity."

On the Possibility of Observing by Transparency, by the aid of a strong Electrical Light, the internal state of certain parts of the Human Body. By M. Eselite.

The perpetual secretary did not venture to say more than a few words upon this, fearing that the letter is either a deception or an act of madness. The author of the letter asserts that he has proved that the human body may be rendered transparent so that everything going on in the interior may be seen. For this purpose, it is merely necessary to place oneself in a dark chamber, and project the brilliant light of the battery upon the part to be observed by transparency. "I have constructed," says the author of the letter, "a dark apartment, pierced with a simple opening, upon which I apply the part of the body that we are desirous of observing. I direct the light of a strong Bunsen's pile, placed outside and furnished with good reflectors, upon this opening; we can then easily distinguish the veins, the arteries, and the nerves; the microscope enables us even to follow the transfusion of the blood from the capillary arteriods into the most slender vesicles." According to the author of the latter, not even the bones present a shade. The power of the illumination is not arrested by a fir-plank a centimetre ($\frac{2}{3}$ in.) in thickness; he has also been able to read a letter placed in the middle of a quire of twenty-four leaves.

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Feb. 16.—M. Arago laid on the table some notes he had received relative to a damsel named Cottin, who was stated to possess some extraordinary attractive powers—*magnetically* attractive we mean. He was induced to be present at a *séance;* he saw the chair in which the damsel was seated shaking violently; nothing, by-the-by, extraordinary this! He could see no action of hers on distinct bodies, as had been reported to him; nor did he see any action of hers upon magnetised needles: we should have been astonished if he had!! There was nothing in the action of her hand upon a piece of white paper "beyond what many persons produce under analogous circumstances." However, M. Arago was induced, we presume from what he had been told, certainly not from what he had seen, to name a committee. We cannot help thinking that the agitators of this phantasm must have laughed in their sleeve, when they saw that "a committee composed of MM. Arago, Becquérel, Isidore, Geoffroy St. Hilaire, Babinet, Rayer, and Pariset were charged to examine what link there might be in the facts announced."

Feb. 23.—"*Note on the Vibrations to which a Galvanic Current gives rise in Soft Iron.*" By M. G. WERTHEIM.—Many philosophers have lately been occupied with the sounds that iron bars or wires give forth at the moment when the circuit of a galvanic current is opened or closed, which is passing through a spiral surrounding the bar or wire in question; or even through the wire itself. This phenomena, discovered by Mr. Page, and verified by Mr. Marrian, has been studied with particular care by MM. De la Rive and Matteucci. These philosophers have made known the best arrangements for rendering the sound very distinct; and they agree in attributing it to a species of shock in the interior of the bar, or to a new arrangement of the molecules of iron. But they are not of the same opinion as to the species of vibrations produced by the sound. M. De la Rive thinks they are transverse vibrations, while M. Matteucci endeavours to prove that they are made in a longitudinal direction.

It therefore appeared to me necessary to examine, first of all, the nature of these vibrations. For this purpose, I fixed a bar of soft iron, 2 metres long and 1 c.m. wide, firmly by its middle. Each half of this bar is contained in a glass tube, sufficiently large that it may oscillate in it freely; and these tubes along their whole length are surrounded by a spiral of copper wire 1 m.m. in diameter. An eye-piece furnished with a cross of wires is placed at one of the ends of the bar, that pass beyond the tubes. The current arises from a pile of 20 Bunsen's elements, and the interruptions are made by means of a rheotome placed in another chamber, in order to avoid all noise and all extraneous movement. After having placed the intersection of the two wires of the eye-piece on a very distinct point marked upon the bar, the current is established. At the same instant the longitudinal sound is very distinctly heard, and the indicatory point is seen to be displaced

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not only in the direction of the length of the bar but also perpendicular to this. This lateral displacement, which is always accompanied by visible transverse vibrations, remains the same, whatever be the position of the bar in respect to the magnetic meridian; but it occurs, according to the different position of this, sometimes on one side, sometimes on the other, or else upwards or downwards.

In order the better to study these movements, I substituted for the two helices, of which he made mention above, a large bobbin, which M. Breguet was good enough to place at my disposal. This bobbin was 19 c.m. in internal diameter, and was composed of 1336 metres of copper wire 2·5 m.m. in diameter. After having placed this bobbin so that its axis was horizontal, and coincided with the axis of the bars, the circuit was closed; the longitudinal sound, although feeble, could however be distinguished, the bar advanced a little in the direction of the axis, and there was no further displacement or transverse vibrations; but, as soon as it was placed out of the centre, it was attracted towards the nearest point of the bobbin at the moment when the circuit was established; and it did not return to its first position until the current ceased; it vibrated at the same time transversely round each of these two positions.

We therefore see that, by bringing the bar near to the corresponding point of the circumference of the bobbin, it may be made to bend horizontally or vertically, or in any intermediate direction.

This is probably what happens in an experiment lately communicated to the academy by M. Guillemin. In this experiment, M. Guillemin saw a bar of iron surrounded by a spiral, and loaded with a weight at one of its extremities, rise up in a visible manner when the circuit was closed. M. Guillemin attributed this movement to an increase of elasticity in the iron by the effect of the current. Now, according to the results of the experiments that I formerly made upon this very subject (Memoir on the Influence of the Galvanic Current, and of Electro-magnetism on the Elasticity of Metals.—*Annales de Chemie et de Physique*, 3rd series, vol. xii. p. 610), the co-efficient of the elasticity of iron, far from increasing, on the contrary diminishes by the effect of magnetization; moreover, the change of bend due to this cause would never have been so considerable as that which has been observed in this case. This experiment, on the contrary, is explained without difficulty by the observations that we have just made; for the bar, when bent by the weight, must have been withdrawn from the axis of the spiral, it must, therefore, have been attracted or repelled by a portion of this spiral: which at the same time enables us to comprehend the extent of this movement; for, on placing the bar sufficiently near to the side of the bobbin, a bend of several millimetres is easily obtained.

It remains for us to know whether the longitudinal movement of the point of observation is due to a real lengthening of one-half of

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the bar, or to a displacement of the entire bar, in this case whether the clamp in which it was pressed had yielded a little. For this purpose, and to give it the greatest possible mobility, I rested the bar upon two glass cylinders. At the moment when the current was closed, the bar was violently attracted toward the spiral, and was displaced at least 1 m.m. It is, therefore, evident that the spiral exercises a sufficiently strong attraction over the bar parallel to its axis. But is it sufficiently intense to produce a visible lengthening? I hope soon to be able to decide this, by means of a micrometric apparatus, the construction of which has been kindly undertaken by M. Breguet.

I then repeated the experiments that have already been made for producing sounds in iron wires. For this purpose a soft iron wire was stretched in a longitudinal sonometer by bronze clamps: thus arranged, it gives the longitudinal sound by friction; and the same sound is heard when the current is passed directly through the wire on a spiral by which it is surrounded. Only in the first case, the sound is a little graver. I did not distinguish any transversal sound, even on placing the wire out of the centre of the spiral. On the contrary, several were heard when an ordinary sonometer was used, but in these instruments, a part of the string that is to vibrate is not exactly limited; the pegs yield, the case enters into vibrations, and nothing but a confused noise is heard,—a babel of sounds, to use M. De la Rive's expression.

All the experiments seem to me to be very easily explained by the magnetic action of the spiral itself, and I think that they may even serve to study, with more accuracy than has hitherto been done, the position of the poles in a spiral, conducting a galvanic current, and the laws of its magnetic office.

In fact, at the moment when the current is established, the bar and spiral become electro-magnets, mutually attractive of each other. I have not seen any case of repulsion, except an experiment with iron filings, which I repeated after M. De la Rive. A part of the iron filings, spread out upon a glass plate, approached the centre at the moment when the plate was introduced into the bobbin perpendicular to its axis.

Moreover, the result would be the same if there were a repulsion instead of an attraction. We may decompose the attraction that each element of the bar exercises over the bar into two forces, of which the one is parallel and the other perpendicular to the axis; in thus decomposing the attractions derived from the elements of one entire convolution of the spiral, we see that all the perpendicular components will be mutually compensated, whilst the parallel components will be added; and it is the same whatever be the number of the convolutions of which the spiral is composed. Consequently, when the bar is placed in the centre of the spiral, it cannot move either on one side or the other at the moment when the circuit is closed, but it is attracted along the axis of the spiral. This sudden traction, which tends to elongate the bar or

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to shorten it, gives rise to the sound in a manner entirely mechanical, as would a rubbing or a blow given in the same direction. We may even produce mechanically, by a single charge of tension, a dry and short sound, perfectly identical with that arising from the action of the current. In fact, we have only to stretch a metal cord very tightly on a longitudinal sonometer, and then suddenly to unscrew one of the clamps; at the moment when the wire is suddenly unloosed the longitudinal sound of which we have just spoken is heard, and this manner of producing it appears to me perfectly analogous with that which occurs at the moment of the interruption of the current.

When two equal spirals are superposed, and the same current is made to traverse them in opposite directions, the horizontal components are compensated, as are also the components perpendicular to the axis; consequently no sound is produced.

Let the bar or wire be now placed out of the centre of the spiral, the horizontal components can no longer be compensated; and the bar, still continuing to give out the longitudinal sound, will be attracted towards the spiral by a force which will depend upon the force of the current, the law of attraction, and the distance of the bar from the centre. The dimensions and the co-efficient of the elasticity of the bar being known, and the angle of deviation being given by experiment, we may immediately express the force of the resultant in weight. It is clear that this lateral traction may produce a transverse sound, which will co-exist with the longitudinal sound. Finally, the origin of the sound by the transmitted current is explained in an analogous manner. For this it is necessary, according to M. de la Rive's experiments, that the wire opposes a certain resistance to the passage of the electricity; it will, therefore, extend suddenly, in consequence of its heating and the diminution of its elasticity, at the moment when the current is passed; and it will regain its former tension when the current is opened. In either case the longitudinal sound must be heard, and this is actually what occurs.

On a Trombe that exercised its ravages at Moulins. M. Gouillard.—The meteor occurred January 20, 1846; the *resumé* of the phenomena is as follows:—If we pass in review the divers facts that we have presented, we see, 1st, that all the objects that were overturned were so only in two directions almost perpendicular; unprotected objects, situated in the open country, fell in the same direction as the current; we know of only a single exception, a tree in M. Mérie's vineyard. On the contrary, all the objects rooted up lay in a direction perpendicular to that of the hurricane. Here we know of no exception. 2nd, The surface itself of the soil, along the whole extent of six killometres, presents absolutely no sign indicating that it had been touched by the hurricane. 3rd, The points attacked are placed in a right line along a length of about six killometres, at distances varying from thirty to three hundred

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metres. 4th, The points attacked at the same time present generally a width of sixty metres; but it is sometimes smaller. It is much greater, on the contrary, at the points, where the meteor disappeared.

March 2.—M. TANCHON announces that he tried in vain, on the 19th and 24th of February, to obtain from the young damsel, Angelique Cottin, any of those electric manifestations that he thought he had observed on the 13th and 14th of the same month.

March 9.—*On the Polarity produced by Electric Discharges, and on its employment for the Determination of the Quantity of ordinary Electricity, associated with the constituent parts of Bodies in combination.* By M. BECQUEREL. Pp. 24—(To be given in our next.)

M. ARAGO gave in *the Report of the Committee relative to Angelique Cottin.*—She had been reported to have overturned tables by the mere contact of a silk thread—*no effect of this kind was seen.* Her arm had been said to make a compass needle oscillate, and leave the magnetic meridian—*nothing of this kind could be detected.* She was reported to be able to distinguish the north from the south pole by the mere touch—*no such thing, she failed in this also.* The only thing she did manage was to shake the chair about violently. But very serious suspicions of some trickery in this were excited; and the committee discovered that this was brought about by certain skilful and concealed manœuvres with the hands and feet. The committee were then told that the damsel had then *lost* her powers; but they did not appear to come back again for the *committee* to see, although she exhibited herself and her doings daily to the public. The committee vote, that all the communications respecting her before the Academy should be considered as not having been presented.

On the Electric Conductibility of Solid and Liquid Bodies. By M. ED. BECQUEREL. Pp. 4.—(To be given hereafter.)

Memoir on Dr. Neef's Experiments, and on the general Theory of Light, Heat, and Electricity. By M. MOIGNO.—(Sent to a Committee.)

On the Vibrations produced by an Electric Current in a Bar of Soft Iron.—Extract of a letter from Prof. DE LA RIVE to M. ARAGO.—(In our next.)

Reply to the Remarks made by M. WERTHEIM, *concerning a Communication by* M. GUILLEMIN, *on the Change produced by an Electric Current, in the Elasticity of a Bar of Soft Iron.*. Letter from M. GUILLEMIN to M. ARAGO.—At the meeting of February 23, M. Wertheim pointed out a cause of error, into which, according to his idea, I must necessarily have fallen in making my experiment upon the elasticity of iron, when subjected to the influence of a current.

M. Wertheim understood that I placed a bar of soft iron to the

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axis of a tube of greater diameter than that of the bar; and that by means of this arrangement the movement which I observed was nothing more than the simple effect of the attraction of the helix upon the temporary magnet. But my experiment was made in a different manner.

Without any thing intermediate I rolled the wire covered with silk upon the bar of soft iron, pressing it as much as possible. There were three convolutions of wire 1.5 m.m. in diameter. In this way all attraction of the helix upon the bar was rendered impossible; and it was to avoid this cause of error that I had arranged my experiment in the manner that I have pointed out.

In fact, long ago, Mr. Perpetual Secretary, I had shown you that a wire traversed by a current attracts iron filings, and small iron or steel needles. I had only one cause of error to fear by this method, which was the following:—

Might not the movement, instead of being a result of the molecular change of the soft iron, be a simple effect of the reaction of the different convolutions of the helix upon each other? To obviate this difficulty, I substituted for the bar of iron a cylinder of dry wood, and with this apparatus I could not observe any sensible movement.

Moreover, in order to satisfy myself that terrestrial magnetism was not concerned in it, I took care to place my apparatus alternately in the plane of the magnetic meridian, and in a direction perpendicular to that plane, taking care to change the direction of the current several times; and the bar constantly raised the weight when the circuit was closed, and let it fall when it was opened.

I had also anticipated the chances of error that might arise from longitudinal attraction in the direction of the axis of the bar within the tube. Indeed, eight months ago (July, 1845) I undertook some experiments to learn what is the greatest effort that may be obtained by means of a bar entering into a helix, employing as a voltaic apparatus only a single Bunsen's pair well charged.

By means of a bar of iron, 3 c.m. in diameter, beat into a horse-shoe form, 50 c.m. in length, the two extremities of which entered into tubes surrounded by four convolutions of wire 3 m.m. in diameter, the length of these helices being 40 c.m., I was able with a similar apparatus, and a single Bunsen's pair, to raise a weight of 1 k.g. to the height of 12 or 15 c.m. in a second of time.

I, moreover, saw that by multiplying the number of these apparatus, rather than by increasing their dimensions, and by making better use of the activity of the outside convolutions of the helix, by means of a hollow iron cylinder forming part with one of the branches of the iron horse-shoe, a much more considerable weight might be raised.

This last experiment was not made on a small scale in the closet of the amateur; I am indebted to the kindness of M. Ar-

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chereau, a clever mechanic, for having been able to execute it, by using the large apparatus that he possesses in his workshop.

March 23rd.—*On the Calorific Power of the Light of the Moon.* M. Melloni.—(Given elsewhere.)

A letter, in which M. WARTMANN gives *the experiments which led him to adopt M. de la Rive's views in respect to the Vibrations in Iron produced by Electric Currents.*

Reply to M. de la Rive's Remarks on a Note concerning the Vibrations to which an Electric Current gives rise in Soft Iron. By M. G. WERTHEIM.—(To be given in our next with M. de la Rive's paper.)

M. DUJARDIN states the new electro-magnetic apparatus, which he described in November 13, 1845, may be used for Faraday's experiments on the action of magnetism on light.

M. BRACHET presents a note *for a new System of Electric Telegraph.*

April 13 —*Can Magnetism exercise any influence over the Circulation of the Chara?* M. DUTROCHET.

April 20.—*Observations on the Influence of Gases in the Electric Effects of Contact.* M. ED. BECQUEREL.—The two principal facts which come philosophers invoke in favour of the contact theory, against the chemical theory of the liberation of electricity, are the following:—

1st. If two plates of the condenser are superposed upon each other, the one being of platinum and the other of gold, and they are then made to communicate by means of a metal arc, a charge of electricity is always obtained: the platinum is negative, and the gold positive.

2nd. If a plate of platinum is superposed upon a plate of zinc, and they are made to communicate metallically, the zinc is positive and the platinum negative; but if, in order to make them communicate, they are touched with the moist fingers, the inverse takes place; the zinc is negative, and the platinum positive.

M. de la Rive gave a satisfactory explanation of these phenomena and other analogous ones, by admitting that the platinum is oxydized in the air, very slowly it is true, and that, by virtue of this action, it is continually negative. He has shown, in support of this view, that electrical effects diminish in proportion as the layer of varnish, by which the air is prevented acting so strongly on the metals, is increased. Thus, according to M. de la Rive, when two bodies in contact are placed in a gas, which exercises upon them a different chemical action, there is a liberation of electricity, as if in place of the gas there were a liquid there endowed with the same property. Having had an opportunity of repeating these experiments, I confirmed their accuracy; and I should not have returned to this subject had I not believed that

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some of the experiments I had made directly proved the action of gases on the metal surfaces under the circumstances in question.

At first sight we have no difficulty in admitting the oxidation of platinum in the open air; however, the observations lead to this result, and it is not the only circumstance in which physics indicate reactions which ordinary chemical processes cannot appreciate. The following experiment shows directly that it is in the condensation of gases, which occur unequally at the surface of metal, that we are led to seek the cause of the electric effects of tension observed in circumstances analogous to those that I have indicated above. If a condenser is formed of two massive plates of platinum, varnished on the opposite faces only, and after a stay of a certain time in the air, they are touched, no action is manifested; but if one of the plates is taken away, and is plunged for a few moments in hydrogen gas, on again placing them opposite to each other, and making them communicate metallically, a very sensible charge of the condenser is obtained: the platinum that has been plunged into hydrogen acquires positive electricity, that which has remained in the open air acquires negative. This effect lasts for a certain time, it then gradually diminishes in consequence of the stay of the plates in the atmospheric air. On each immersion of the first plate in hydrogen the same effects are observed: the plate covered with oxygen always acquires negative electricity.

This experiment very plainly shows that when we operate with a plate of gold and another of platinum, the gold having a less condensing power for gases than platinum, acts as the platinum plate covered with hydrogen, and must acquire positive electricity; this, in fact, is what occurs.

If we cover the entire surface of the plates with gum lac varnish, then the electrical effects diminish, when one of the two plates are plunged into hydrogen gas; it is probable that, with a sufficient thickness of varnish, the electrical effects would cease, as in M. De la Rive's experiments.

If we now consider the results obtained by placing in metallic contact a plate of platinum and one of zinc, we see that the plate of zinc cannot become covered with condensed oxygen, for this gas forms, at the surface of the zinc a film of oxide, which preserves the latter from all ulterior change. The zinc must, therefore, act as a metal having no gas condensed, or as platinum plunged into hydrogen; it acquires positive electricity, and the other negative. If, on the contrary, the two plates are made to communicate by means of moist fingers, the zinc is oxidized by the water that moistens them, and the chemical action resulting from this reaction gives to the zinc the negative and to the platinum the positive; this, in fact, is what is observed. We see, therefore, that gases condensed by metallic surfaces may give the electrical effect of tension, as they give currents when the metals are plunged into liquids.

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The fact being established, it remains to know how we can theoretically conceive that two platinum plates, one having condensed oxygen, and the other hydrogen, or a less quantity of oxygen, the former acquire negative electricity, and the latter positive. We may explain this by admitting that condensed gases do not act the same as when they are at the ordinary pressure; and, as M. De la Rive announces that the oxygen tends to combine with the platinum, this latter must, therefore, acquire negative electricity.

This mode of viewing it is not contrary to known facts, when we think of the exceedingly small quantity of chemical action necessary to produce a sensible effect of static electricity, and which no ordinary chemical process is able to detect. In fact, as my father lately proved,* the oxidation of a quantity of hydrogen, sufficient for 1 m.m. of water, would suffice to give 20,000 charges to a coated surface of 1 m. square, the sparks taking place at 1 c.m. distance. Thus, according to this, on account of the atomic weight of platinum, 1 m. m. of this metal, in oxidating, would give nearly 2000 charges of the same intensity. Now, in order to charge a condenser, without a sensible spark, and so as merely to make the gold leaves of the electrometer diverge the fraction of electricity necessary, would be much below $\frac{1}{10000}$ of the charge. Admitting even this number as the superior limit, we see that the oxidation of 1 ten-millionth of a milligramme of platinum would be sufficient to charge the condenser 2000 times.

Thus, these observations would therefore confirm this fact, that the action exercised upon platinum by condensed oxygen is probably due to a chemical action; and it seems to me that we need no longer quote the effects of which I have spoken against the electro-chemical theory, which accounts for all the circumstances of the liberation of electricity.

M. De la Rive of Geneva, who assisted at this meeting, offered a few remarks upon the works of M. Ed. Becquérel. He shows the importance of the results obtained by this young philosopher, thanks to the accuracy and the care that he has employed in his manner of operating. With regard to the nature of the action that oxygen exercises upon platinum, M. De la Rive is disposed so see in it a chemical action rather than a simple physical adhesion. He cites, in support of his opinion, facts which prove that platinum, exposed successively to the action of oxygen and hydrogen, and this a great number of times consecutively, is finally disaggregated upon the surface; which can be no otherwise explained than that by the effect of these repeated actions, it experiences an alternation of oxydations and reductions.—[A.]

* M. Becquérel's memoir here referred to will be in our next.—Editor.

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NEW BOOKS.

Eenige woorden over het galvanismus, des Telfs toepassingen en de electro-beweegkracht. J. Stein, Antwerpen, 1844 *un* 8*vo.*— *Metodo originale italiano di elettrodoratura; Grinelli, Modena,* 1844, 8*vo.* — *Cenni storica intorno all' elettro-magnetismo considerato come forza motrice. Majocchi, Milan,* 1844, 8*vo.* — *Galvanometro vniversale o a forze variabile. Majocchi, Milan,* 1841, 8*vo.*—*Expériences sur les Piles Seches. Delezenne, Lille,* 8*vo.*—*Météorologie électrique. Peltier, Geneve,* 8*vo.*—*Des conditions nécessaires à la production des courants voltaiques; Majocchi, 2nd memoir,* 8*vo.* — *Medical Application of Electricity, with description of Apparatus, &c. pp.* 21, 12*mo.* 1846. *Boston.*

NOTICES OF EXPERIMENTS, &c.

Colchester, April 11*th,* 1846.

Magnetism and fermentation.—" I am induced to write to you from a conversation I lately had with a very intelligent gentleman who superintends a brewery here, who has some glimmerings of electricity, and whose premises I went over the other day. In the course of my inquiries respecting the process of fermentation I learned some facts connected with it which I fancy have some connection with your favourite science.

" In the first place, I should tell you that the process is begun in what are technically called *squares;* that is, one side of a room is divided (in the case to which I refer, at least) into three divisions by wooden partitions, and these squares are made capable of containing a larger or smaller quantity of liquor by the addition or removal of boards fitting in front. Now when these divisions are all filled, the process of fermentation always commences on the outer sides, and gradually extends over the whole surface. On my inquiring the point of the compass to which those sides bore reference, I found it was due north, which made me fancy that magnetism had perhaps something to do with it; and, not knowing whether any one had ever noticed that peculiarity before, I thought it might not be amiss to let you into the secret, and perhaps you might think it worth notice.

" I should not forget to mention that though it has always been considered that heat is necessary to fermentation, yet in this case it does not appear so; for though the first square is exposed to the greatest heat of the three, from being situated next the boiler, yet fermentation always begins in No. 3, the coolest.

" If, however, No. 3 only be filled with liquor, and the other two with water, fermentation begins on the side marked N, and

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will not extend over the surface till the square next to it be emptied of its contents, when the process goes on as usual."—*Extract from a Note from Mr. E. Mayo.*

Lightning.—An appalling instance of the effects of lightning occurred by which one poor fellow was instantly killed and another had a most miraculous escape from death. It appears that as a number of men employed by Mr. Etches were at work in a field on the London road, Derby, a heavy thunder shower came on, and two of the men took shelter under a tree, where they had been only a few minutes when they were both struck by the electric fluid. One of them named Sandars, was killed on the spot, and the other, John Pegg, was knocked down and rendered insensible. Mr. Fearn, surgeon, was quickly in attendance, and found the survivor in a state of great collapse. Appropriate restoratives were made use of, and he is now pretty nearly recovered. There are several points of interest connected with the narrow escape of Pegg. The electric fluid appears to have struck him first on the top of the head—it pierced his hat by a perforation in the crown not larger than a small shot hole, and rent the hat lining into shreds. The hair on the crown and left side of the head was burned, and the skin on the left side and back of the neck scorched. Then the lightning came in contact with his silver watch-guard, which it passed along, completely fusing the links. The watch-glass was broken, and three joints of the outer case of the watch were also fused. It thence passed down the right thigh and leg, which it scorched very much in its course, to his boot-heel, which was nearly wrenched off. The watch, which was found to be gaining very much, has since been sent to the watch-maker, and the works being to all appearance not at all injured, he was much puzzled to find out why it should gallop on at such a rate, and he at length discovered that all the steel parts were rendered magnetic. Pegg has no recollection of anything happening to him at the time of the accident.—*Derby Mercury.*

Effects of Lightning.—During a storm the house of Mr. Thomas Smith, at Brabourn, was struck by lightning, occasioning much damage, and placing the inmates in great peril of their lives. It appears that the family were in bed, when just before 10 o'clock Mr. Smith was roused by hearing a tremendous crash in the adjacent room, in which five of his children were sleeping; on reaching the room he found the chimney levelled from the top to the floor, the bricks and rubbish nearly covering it, and some portion of the bed. The children were fortunately unhurt with the exception of one, the eldest, a boy about 13 years of age, who received a severe contusion from one of the falling bricks on the left eye. Their escape is almost miraculous, as one of the bedposts is shivered into splinters, and the pillows were actually driven from under the heads of the children, one entirely through the door, the pannels of which were forced out, and the other left hanging in

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the aperture. The bed-clothes were afterwards found to be ignited in two places, each about the size of half a crown, but by timely attention further calamity was prevented. On afterwards examining the premises, it was found that the electric fluid had passed down the stairs, through the back washhouse, to the hog-sty, wherein were two fine animals, weighing each about seven score; one was struck dead, and the other escaped unhurt. The house is so much damaged that it is thought it will be necessary to take it entirely down. Hundreds of persons visited the premises on Monday, many taking away small portions of the shattered bed-post as mementos of the remarkable occurrence.—*Kent Herald.*

Electric Spectral Images.—"Place a key or any other opaque object before a sensitive phosphorescent surface, and having made that surface glow intensely, by a galvanic discharge between charcoal points, continued for two or three minutes, on removing the key an image of it will of course be seen. This image in a short time, will disappear. Then shut the plate in a dark place, where no light can have access but in the day time. If in a day or two, the surface be carefully inspected in the dark, no trace of any thing will be visible upon it; but, if it be laid upon a piece of warm iron, a spectral image of the key is suddenly evolved."—DRAPER. *Phil. Mag.* Dec. 1815, p. 437.

Magnetism and Heat.—"An analogy between magnetism and heat consists in the rays emanating from the former principle being absorbed by the magnetic metals, these thus becoming magnetized just as bodies become warmed by the absorption of heat; but there is this difference between the two agents, viz., that there are but few bodies capable of absorbing the magnetic rays, whilst there is scarcely one which is not able to absorb those of heat."—HENRICI. *Poggendorff's Annalen*, vol. 64.

Magnetism and Crystallization.—"After the lecture Mr. Weekes introduced a singular and interesting form of experiment, showing the complete reduction of nitrate of silver from its aqueous solution, and its tendency, when placed in the plane of the magnetic meridian, or within the influence of a powerful magnet, to pass in the direction of the magnetic poles, where the silver accumulated in a beautifully revived form," &c. &c.—*Proceedings of Canterbury Phil. Inst.*, 1829.

Antiquity of Mariner's Compass.—Mr. Wright pointed out a remarkable instance of this—the mariner's compass, which has generally been looked upon as an invention, if he remembered right, of the fifteenth century; yet documents had recently come to light which alluded to it as being in common use among sailors in the twelfth and thirteenth centuries; and these documents were of the most popular kind, such as songs and poetry, which show still more how well it must have been known. One of them was written in England about the time of king John. In one of these it is said that the sailors who go on long voyages to Friesland, or to the east, know their way by observing the *tramontaine*, or

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polar star; but when the sky was covered with clouds, and they could no longer see the stars of heaven, they had a contrivance, which was this; they took a needle of iron and put it through a piece of cork, so that one end remained out, which they rubbed with the loadstone, and then they placed it in a vessel full of water, and whichever way the end of the needle pointed, there, without any doubt, was the polar star.* This was a primitive but a perfect mariner's compass.

Electric Illumination.—Mr. G. Shaw briefly addressed the audience upon the nature of voltaic electricity, which he illustrated by a number of exceedingly beautiful experiments; and concluded by exhibiting a model of the new electric light invented by Mr. Starr. He stated that this invention furnished the means of applying electricity to the illumination of buildings, &c., and that the permanent magnet would be employed as the source of electricity, which reduces the expense to the small amount of

* Mr. Wright has since favoured us with the following copy of one of the poems alluded to:—

" La tesmontaine (*sic*) est de tel guise
Qu'ele est el firmament asisse
Où ele luist et reflambie.
Li maronier qui vont en Frise,
En Gresse, en Acre ou en Venisse,
Sevent par li toute la voie:
Pour nule riens ne se desvoie,
Tout jours se tient en une moie
Tant est de li grans li servisse,
Se la mers est enflée ou koie
Jà ne sera c'on ne le voie
Ne pour galerne ne pour bise.

" Pour bise ne pour autre afaire
Ne laist sen dout servise à faire
La tresmontaigne clere et pure;
Les maroniers par son esclaire
Jete souvent hors de contraire
Et de chemin les asséure;
Et quant la nuis est trop oscure
S'est ele encor de tel nature
C'à l'aimant fait le fer traire,
Si que par forche et par droiture
Et par ruille qui tous jours dure
Sevent le liu de son repaire.

" Son repaire sevent à route
Quant li tans n'a de clarté goute
Tout chil qui font ceste maistrise,
Qui une aguille de fer boute
Si qu'ele pert presque toute
En .j. poie de liége et l'atise
A la pierre d'aimant bise,
S'en .j. vaissel plain d'yaue est mise,
Sie que nus hors ne la deboute,
Si tost com l'iaue s'aserise;
Car dons quel part la pointe vise
La tresmontaigne est là sans doute."—Ed. L. G.

Meeting of British Archæological Association, March 18.—Vide *Lit. Gaz.*, March 21.

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mechanical power required to keep the armatures in rotation. The steadiness and brilliancy of the light were greatly admired by those present, as was evidenced by the warm applause with which it was received; and as this was, as we learn, the first occasion of its public exhibition, it has attracted attention.—*Birmingham Lit. Lec.*, May 11.

Iridescent Silver.—"A few months ago, while engaged in some experiments in electrotyping, I was led to think that, by this process, the hues of the pearl (mother of pearl) might be readily transferred to those metals, which from their hardness, are incapable of receiving impressions in mass; but yet, on account of their freedom from oxidation, retain for a long time a surface comparatively pure. I therefore took a Smee's battery, which I had just constructed, and, after several experiments, succeeded in obtaining small sheets of silver, radiant with the hues of the shell. When seen by a single light, as that of a lamp, the play of the colours is surpassingly beautiful, scarcely inferior to that of the pearl; and, where equal care was employed, the plate of silver, which was formed eight months ago, rivals in brilliancy that which came fresh from the battery a few hours since.

"The process by which this result is obtained is as follows:—The first thing required is to prepare the shell. This is effected by grinding, and polishing it upon the back, in such a manner as to cut through the numerous concentric strata that compose its substance. When this is done, by the aid of a microscope the surface will be seen covered with delicate grooves, some thousand in an inch, formed by the sections of the concentric laminæ; and this configuration gives rise to the glowing tints of the shell. The next step is to obtain an exact impression of their surface upon some good conductor of electricity. This we are enabled to do, by means of fusible metal, if proper precautions are employed in taking the impression. I pursue exactly the same method as in taking the copy of a medal. After fusing the metal, I pour it upon oiled paper; and when the air-bubbles cease to rise through the metal, the oxide is skimmed from its surface with a cord; and, as soon as it presents the appearance of a perfect mirror, the shell is forced down upon it by a sudden pressure. When the metal is cooled I remove it from the shell; and, having ascertained the accuracy of the impression, immediately plunge it, before any change of the surface can occur, into the silver solution, thereby completing the circuit between the poles of the battery. In a few moments the surface of the metal is frosted with silver, and the configuration of the shell exactly copied. A sheet of silver, of sufficient thickness to be easily removed with a penknife, will be deposited in the course of five or six hours under favourable circumstances. By the process above described, we can at pleasure transfer the tints of the pearl to those pure metals which will best preserve their brilliancy; and, while the knowledge of this fact is interesting as a matter of science, it may perhaps be well for the

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artist to consider if it cannot be applied to some ornamental purpose, and the beauty of the precious metals enhanced, by teaching them to glow with the richest hues of light."—*Note from Prof. Brocklesby to B. Silliman, jun. Journal for* 1846.

Lightning.—The season up to the present time has been one of the most propitious—if fair, fine hot weather is propitious—that we ever knew to have occurred. From the middle of May to the middle of June there has been one incessant broil—a fierce sun almost without a cloud. The change, as might have been expected, was accompanied by an abundant display of lightning, and its attendant thunder. The phenomena consequent on the change very well illustrates one of the recognised causes of the formation of storms:—a sudden and extensive mixture of two masses of air, at different temperatures, and differently charged with moisture. In the present case the mixture occurred,—an enormous mass of water was precipitated,—attended by violent thundering; and when all was over the weather became for a few days very cold: the coldest wind prevailed, and so wrought an effectual change in the temperature. The barometer had fallen. The weather soon again changed to warm, and is now very genial and fine. It is curious to regard the sudden transition which thus occurs from more than tropical heat to autumnal cold,—it is almost like a transposition to another clime. Cases do occur, in which the warmer wind triumphs in the end, and then we have the thunder and the thunder-showers without the subsequent cold. The damage done by the lightning has been severe in many places.—C. V. W.

Omnia.—Extract from De la Rive's lecture on the progress and the applications of electricity, as read before the Helvetic Society in last August, is given in the fourth and subsequent numbers of the present volume of *L'Instirut.*—On Jan. 21st was read, at the Society of Arts, a paper by Mr. Nott, on the connection between the photographic agent and electricity.—H. R. H. Prince Albert is the patron of the Royal College of Chemistry, lately formed in London. — Mr. Sturgeon has communicated to the Royal Society a successful repetition of Dr. Forster's experiment on electro-culture.

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ELECTRICAL MAGAZINE.

OCTOBER, 1846.

CLXII. *On Electric Polarity, in its relations with Light and Heat.* By DR. NEEF.*— Archives des Sciences Physiques,† &c. Vol. i. p. 30.

(TRANSLATION.)

1. IN all that concerns the relations of electricity with the other natural forces, there are certain primitive facts which, like the bud of a flower, contain within them an entire organism. In studying these facts attentively, we arrive by a succession of isolated phenomena, to get an idea of the whole. This, for example, is what has happened in the decomposition of water, in relation to electro-chemistry, when oxygen was observed to appear at the positive pole, and hydrogen at the negative; the same was the case in regard to electro-magnetism, when Œrsted discovered that the magnet takes a normal position in relation to the direction of the electric current, inasmuch as its polarity is in a determinate relation with that of the current. In like manner, in other parts of the science of electricity, it is necessary that

* Dr. Neef's memoir appeared to us calculated to be of more lively interest to our readers, inasmuch as it preceded but a short time Faraday's excellent work on the relations existing between light on the one hand, and electricity and magnetism on the other. We do not in all points entertain Dr. Neef's ideas; and we shall in a very short time have an opportunity of making known by what differences we are separated. But, independently of certain curious facts, his memoir contains certain new and very interesting considerations, which, if they cannot be entirely admitted, are at least of a nature to make men who are devoted to the study of physics to think and work in a fruitful manner.—(A. d. l. R.)

† This is the title of a publication into which the *Archives de l'Electricité* have, we are sorry to say, been converted.—ED.

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the researches of philosophers be supported on fundamental experiments, the exact and methodic observation of which can alone lead to satisfactory consequences and deductions. In like manner, for the production of light and heat by electricity, there must be primitive phenomena, which, when freed from all that may complicate them, present the elementary facts in a manner at once clear and simple. In studying the properties of electric poles, I have observed phenomena that appear to me to have this character; I will make them known in order that they who possess more extensive resources than mine may complete the study of them.

1. *On the Origin of Electric Light.*

2. Electric light is regarded as the result of the neutralization of two electric principles produced between the poles in media that are bad conductors. With regard to knowing whether one or both principles give the light, or if the light appertains to them as much as do the fluids, or if it is the electric medium that becomes luminous, and finally, what part heat holds in the production of electric light, these are so many questions upon which our ideas are not sufficiently clear, and which have not yet been the subject of satisfactory experiments. The only point upon which there is agreement, is in admitting that to the luminous principle there is a movement that is represented as a current, having a single direction, or two opposite directions, and appearing under the form of sparks, in the production of which the two poles concur, but more particularly the positive pole.

3. The causes assigned for this want of clear ideas upon the nature of this phenomenon are principally found in its short duration, and in the part played by heat. If the luminous discharge is an isolated light, it occurs in an instant so immeasurably short that it cannot be perceived. If it is composed of an extremely rapid succession of innumerable sparks, as in the case of the electric pencil that escapes from the conductor, the polarity of the luminous substance remains uncertain; for we may consider the luminous source to be either the positive electricity that is liberated, or the medium, that is electro-negative by induction; and the same may be said in respect to the other pole, changing the two electricities. Finally, if the phenomenon is accompanied by a very powerful liberation of heat, as occurs in violent discharges of the Leyden jar, or in the phenomena of combustion of the voltaic battery, the production of light acquires

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a complicated character, and becomes almost entirely secondary; it must then be attributed to the particles of metal or carbon in a state of incandescence, almost as certainly as the platinum wire heated between the poles of the battery is attributed only indirectly to electricity, and immediately to heat. But one thing is certain, that there exists a primary electric light, which is not produced by the aid of heat, in the same manner as there is a dark electric heat. A great number of the electrical phenomena of friction present this electrical light in an extreme degree of vivacity, with a minimum degree of heat. It is only to be regretted that the same circumstances, in this as in the other case, prevent the successful study of the origin of this light, and of extricating it from the midst of the chaos of phenomena.

4. I have succeeded in attaining this end by another process, which I am about to make known. With magneto-electricity,* under favourable circumstances, we may see the electric light produced in a very distinct manner, free from all embarrassing complication; it may be pursued to its origin, and the nature of its polarity may be recognised in an unequivocal manner. Indeed, as this source of electricity acts with a greater intensity, and a smaller quantity of the voltaic pile, it furnishes more primary light and less heat than this apparatus; it approaches more closely to the electricity of friction, but is less superficial, and exercises a more energetic chemical action. It, therefore, presents for the researches in question a happy medium between the two other extreme cases. We may, moreover, increase or diminish it at pleasure in respect to intensity and quantity, according as a larger or thicker helix is taken. But the chief advantage presented by this source of electricity is, of permitting us better to distinguish, and to carry to a higher degree of intensity, without confounding them, the effects of polarity, which, with the voltaic current, escape the experimentalist, on account of the rapidity with which they neutralize each other.

5. I made use of my magnetic electromotor,† as the apparatus most suited to these experiments, because, by aid of this, we are able to produce both isolated magneto-electric discharges, and a numerous and extremely rapid succession of the same discharge. Moreover, the place where the discharge occurs remains always the same, and

* I understand by this the electricity that is produced by the alternate origin and disappearance of magnetism, not only the permanent magnetism of the steel magnet, but also with the momentary magnetism of the electro-magnet.

† *Poggendorff's Annalen*, vol. xlvi. p. 104.

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consequently renders an accurate observation easy. In its mode of construction, the apparatus differs slightly from that which I have elsewhere described; in particular, the luminous effect is not hidden, as is the former, by a blunt point. My apparatus resembles that of Desaga, the mechanician of Heidelberg: the hammer is covered with a small leaf of platinum, against which at right angles rests the conical point of a platinum wire, communicating with the anvil. We thus have the advantage of seeing the luminous phenomena in all directions without obstacle; and of being able to observe it and determine its intensity either on a plain surface or on a body of a conical form. As an exciter, the most suitable instrument is the battery that I have described elsewhere. In most cases, a single pair is sufficient. If we are engaged in experiments in which the effect diminishes, or in which more energetic effects are in view, we may connect two, three or four pairs together, by putting them in series. With a source so moderate as this, all the effects are simply magneto-electric; by ever retaining the circuit constantly closed, we are not better able to redden the point of the platinum wire than we could by means of a rapid succession of interrupted contacts.

6. We know that, when the apparatus acquires its oscillating movement, there is produced at each break of the circuit a spark between the hammer and the anvil; and that this luminous phenomenon acquires an appearance of continuity due to the rapidity with which the interruptions succeed each other. It is an error to take them for true sparks passing from one pole to the other, or always to attribute them, as I have myself down, to metal in combustion. Combustion occurs only when the electricity acquires great energy; when its quantity is feeble and its tension considerable, the luminous phenomenon is accompanied with only a very small amount of heat. By weakening the ordinary light of day, the spark may even be perceived by the naked eye under the appearance of a violet light, whether the current be directed from the hammer to the anvil, or whether it travel in the opposite direction. But it is altogether not very brilliant; and thus was I led to the idea of examining it with the microscope.

7. On observing it with this assistance, it is immediately manifest that *the luminous phenomenon always appears at the negative pole,* namely, that it is the platinum point of the hammer which becomes luminous when the current passes from the hammer to the anvil; and the platinum plate of the anvil when the current pursues the contrary direction. It is a known fact that when the circuit is opened, the magnetic

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electricity follows the same direction as the primary current; it also decomposes water in the same order; it liberates oxygen on the electrode coming from the copper of the pile, hydrogen on that leading from the zinc, it is therefore beyond doubt that this latter is the negative pole. For further information on this subject, M. Gassiot's demonstration may be consulted.

8. A lens, magnifying five times, is quite enough to enable me to recognise in an unequivocal manner this fundamental phenomenon. But to distinguish it in detail, it requires a higher magnifying power. Those which I have found the most advantageous have magnifying powers of about twenty-five or fifty times, with one of Plössl's microscopes, for which I had a horizontal support made; with these magnifying powers we are able support the object at a distance of $1\frac{3}{4}$ inch from the object-glass. With a magnifying power of sixty-six times, we do not see any the better, and the image is less distinct. Moreover, it ceases to be necessary, and indeed it is a disadvantage much to darken the field of vision; for example, when the point becomes luminous, it is reflected to the surface.

9. Thus provided, the eye clearly distinguishes that the light originates in two distinct sources. The first source is an assemblage of *small white points* of the most vivid brilliancy, of an inappreciable degree of smallness, since with a magnifying power of sixty-six times, no real diameter can be assigned them, any more than there can for the fixed stars with the most powerful telescope. These small points adhere strongly to the platinum, whether the surface of the anvil or the conical point of it plays the part of negative pole. They never show themselves free in the light luminous envelope, which will be spoken of further on, nor yet in the air outside this envelope. They are seen sometimes on one portion of the surface, sometimes on another, according to the change of place which the explosions themselves undergo. Most frequently they are at the extremity of the pole, consequently at the point of contact; on the point of the luminous cone, as it leaves the anvil, or finally at the middle of the luminous disc near the hammer. These points are formed of small and extremely fine particles of the surface, (which is always slightly rough) which emit a concentrated light, to which is given the name of the *points-light*, in frictional electricity: thus, when we use a perfectly polished needle, it is only perceived quite at the extremity.

10. I shall call the other luminous source, flame, on account of its resemblance to a feeble permanent flame of a violet

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colour, and for other reasons, which I shall mention hereafter. If it appears on the anvil, it surrounds the platinum point under the form of a luminous envelope. As a mean, I found its length 0·5 mm., and its thickness 0·04 mm. 0·05 mm. But, if it appears on the hammer, it is placed horizontally on its surface, presenting the appearance of a disc, $\frac{1}{2}$ a mm. in diameter and $\frac{1}{25}$ to $\frac{1}{20}$ of a mm. in thickness.

11. It is plainly evident that the whole of the visible phenomenon originates in the negative pole, and forms around it a considerable luminous envelope. It cannot be denied that there may also be developed a more feeble light at the positive pole, only it cannot be perceived on account of its being confounded with the luminous sphere of the negative pole, in which it exists as a nucleus; but we have no means of preventing this confusion; and it is only by analogy with what passes in frictional electricity that we can sustain the conjecture that we have just put forth.

12. We have as yet always supposed that these observations were made by means of an oscillatory movement of the magnetic electromotor. This, in fact, is the best means of getting a good view of this magnificent phenomenon. It presents itself with this instrument in so permanent a manner, that we may consider and appreciate it at one's ease for more than a quarter of an hour. But the more uniform the flame remains when it is at a moderate intensity; the more changeable are the brilliant points; they appear to be confusedly agitated. When we reflect that what appears to us to be continuous is really formed of an extremely rapid succession of innumerable little lights, we recognise without difficulty that the movement, which these points present to us is an optical error, due to their being very numerous and to their reflecting the light in turn.

It is, consequently, necessary to observe also one of these lights isolately; and this is managed without difficulty, by directing the hammer with the hand, instead of allowing it to move by the play of the machine. If we have not at our disposal an apparatus similar to that of which I have spoken, it is sufficient to procure a simple voltaic pile, a helix, and a lens magnifying about twelve times, to see distinctly both the polar phenomenon and that presented by the isolated lightnings: we see, in fact, the flame and the white points illuminated at the same time; but these latter are fixed, they do not move in any direction; these are not sparks.

13. If we wish to study this phenomenon better, we must gradually *reduce* the magneto-electric intensity which hitherto we have supposed to be constant. The best means of obtain-

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ing this effect is to make use of a moderator.* With this view we place between the magneto-electric poles a glass cylinder filled with water, and one of the poles is thrust more and more into the liquid; the light is thus gradually reduced until it disappears entirely by the too great length given to the liquid conductor. At the same time the violet flame becomes more feeble and thinner, and its light, consequently, less vivid. With regard to the small, shining, white points, their number diminishes; afterwards they are only perceived at the place of contact, where they finally also disappear. If the length of the interposed liquid path is gradually diminished by the moderator between the electrodes, the two lights again increase in intensity. If the intensity of the luminous source is diminished, so as no longer to have more than a few luminous points, the violet flame can no longer be perceived, so that the more feeble the phenomenon is, the whiter is the light; the more intense it is, the more violet is the light.

We are now concerned in knowing if the flame really disappears altogether when the white points are still glistening; and if, consequently, it only shows itself when the magnetic electricity attains a certain degree of intensity; or if in this case it only becomes imperceptible to the sight. A circumstance favourable to this latter hypothesis is, that the light of the flame is of itself much less intense than the concentrated light of the white points, and that the thinness of its luminous film, which is only the tenth of its diameter, may very readily, when the phenomenon is at its minimum, render it invisible, without, however, the phenomenon really disappearing. I do not dare to decide whether the flame ought be placed in the same line as the luminous points, or whether the latter really constitute a primary phenomenon, or whether the flame is only a secondary phenomenon.

14. Here is another mode of reducing it, which I have obtained by an experiment as instructive as the rest. I divided the luminous phenomenon by fixing with its head a very fine steel needle upon the hammer of a second magnetic electromotor, so that its point rests against the platinum plate of the anvil placed opposite; this was, therefore, an inverse arrangement to that of the former apparatus. The magneto-electric poles of the latter were placed in communication with the hammer and anvil of the second. Now, whilst the first was executing its oscillatory movement, the needle of the second also underwent oscillations, and entered thus into frequent contact with the anvil, which was very near to it,

* *Pogg. Annalen*, vol. i. p. 236.

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and presented the luminous phenomenon in consequence of a derived current. The needle being much finer than the platinum point of the principal apparatus, took away from this point only a small portion of its electricity, so that the light which it emitted was much more feeble. But, by means of a magnifier of twenty-five times, I could observe in a distinct manner that, when the needle served as negative electrode, it presented only a single luminous point, and that the point was quite at its extremity, a circumstance due, as I have said, to the polish of the needle, the flame was again violet; thus the difference of the metal upon which it was produced did not induce any difference in the colour. Besides, it was larger than on the platinum point, but its light was much less brilliant; frequently it was not at all visible. When it appeared, the whole phenomenon resembled a comet. When the anvil was negative, and it became luminous, I also saw the flame produced, but very rarely; in ordinary cases, only a brilliant disc was seen formed by innumerable small luminous white points.

15. When a small drop of water, or alcohol diluted with water, is placed between the hammer and the anvil, the oscillations of the apparatus did not cease, it is true; but if the electricity is feeble, there is only a liberation of gas, due to the decomposition of the water, and the luminous phenomenon does not occur. In order to produce it in this case, the electricity must be very intense. If the water is allowed to evaporate slowly, the white points are first seen to appear, and then the flame. It is impossible to see, in a more beautiful manner, this nascent state of the electric light.

16. But the *increase* in the intensity of the phenomenon is not less instructive than its decrease. In order to obtain it, a more powerful pile must be employed, but such that it may not make an electrode become red hot, because it is always the primary light that we wish to study. Two or three piles connected so as to form a voltaic series fill this end very well. The effect produced consists in the brilliant points being in greater number, and the flame being greater. But these we also see appearing on the platinum plates, when the white light presents itself in the form of discs of a sensible diameter. And the flame is not a quiet luminous envelope, but it frequently darts to a good distance beyond its limits, sometimes with a red colour; by the microscope it might be called a veritable luminous volcano. From time to time there are also seen small sparks, properly so called, namely extremely small luminous points, which dart from the luminous electrode, so that they become visible, and extend beyond the flame; but

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they take no decided direction on the side of the dark electrode; they are not sparks that traverse the interval from one electrode to the other.

17. If the intensity of the electricity that is developed is considerably augmented, it is no longer possible to prevent the heat that is produced from determining a state of incandescence. Then we are no longer concerned with the mere primary light; the light derived from incandescence and that due to combustion differ essentially, and do not depend directly on electricity; the two kinds are so confounded that I have not yet been able to distinguish them sufficiently. I will, however, speak again of certain particular phenomena related to this subject, and of divers other circumstances relative to the production of light by electricity, but which cannot at present be entirely comprehended.

II. *On the Origin of Electric Heat.*

18. As the luminous phenomena are only manifested at the negative pole, I thought within myself of the kind of correspondent action which must be exercised at the non-luminous positive pole. I presume that this must be a calorific action. It is well known that, as soon as the electro-motive force commences being felt, it produces heat in its conflict with the resistance of the conductibility of the metal,—its higher degree of development is manifested by ignition. This liberation of heat occurs either by the discharge of the Leyden jar, or in the continuous circulation of the electric current with the wire that closes the voltaic circuit, or by the discontinuous action of magnetic electricity in its passage through good and bad conductors. But when the circuit is closed by metals, the particular properties cannot be distinguished at each pole, especially in what concerns the development of heat. There are only three methods for rendering evident the polar effect in this respect; 1st, the employment of magnetic electricity; 2nd, the effect of the current accompanied by light, which is established at a distance between the electrodes of powerful batteries; 3rd, the decomposition of water by very energetic batteries. Each of these means produces a powerful circulation of electricity, still maintaining in very distinct opposition on either side, an excess of polar forces in a state of liberty.

19. *First Method.*—My attention being directed to the phenomena presented by the positive pole of the electromotor, I perceived during an experiment that I related above (14), a phenomenon that confirmed the opinion which I held, that the anode or positive electrode is the source of heat, as the

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cathode, or negative electrode is the source of light. I have mentioned how the fine steel needles employed as cathodes became luminous under the influence of feeble electricity. On repeating this experiment, I have sometimes seen under the microscope, when the electricity was a little more energetic, the point of this needle suddenly pass to a glowing red when it was made to serve as an anode, that it was not consequently luminous, and that the negative anvil emitted light opposite to it. The phenomenon never occurred if the point were negative. In order that the experiment succeed, it is necessary that the point be extremely fine, so that it easily became red, even under the influence of feeble electricity; but at the same time that it does not communicate its heat to the metal surface of the anvil placed opposite to it, and does not produce in it any disturbance; for if the two electrodes present equal masses, the ardent heat of the positive electrode is propagated to the luminous negative electrode, in virtue of calorific transmission. The experiment, however, has no chance of succeeding unless the requisite degree of electric intensity is attained.

20. Walker made the following experiment with his powerful battery consisting of 160 Daniell's cells.* He superposed, crossing them, the polar wires of the battery, but without placing them in contact, leaving between them a small interval. A continuous current of brilliant light was immediately established through the stratum of air; at the same time this singular circumstance was remarked, that the positive wire became a deep red from the point of crossing to its free extremity (consequently even out of the circuit), which is softened and bent, whilst the negative wire remained relatively cold.†

21. *Third Method.*—Walker plunged the polar wires into two vessels filled with water, in which thermometers were placed; the communication between the two liquids occurred by means of a moistened capillary wick. The liquid in which the positive wire was placed always presented a superior temperature to the other.

22. This in every way confirmed the principle that electric heat has its origin at the positive pole. It still remains, as the end of these researches, to multiply these experiments, to repeat them by varying the conditions under which they are

* Transactions of the London Electrical Society, p. 65-71. *Pogg. Annalen*, vol. lv. p. 62.

† We think that this experiment is due to Mr. Gassiot, and not to Mr. Walker. —*De la Rive.* All the share I have in it is being present, and taking notes of the experiment, from which notes Dr. Neef has gathered his information.—C. V. W.

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brought about, and to demonstrate the existence of the same law in other known facts.

23. It is known that electric heat, which in itself is nothing else than a diminution of the force of cohesion, is accompanied by a mechanical effect of electricity, which produces a direct destruction of the state of aggregation, then disaggregation, until the rupture of the body and its reduction to dust. An excellent work on this subject by Mr. Reiss* has just appeared, in which the author describes in the following manner the succession of effects, at once thermic and mechanical, of the electricity upon the metallic conductor, setting out from the most feeble development to the most violent discharge; the wire becomes hot, it separates violently from the apparatus destined to fix it, it explodes, it melts, and goes to powder.

24. Now this *cessation of the state of aggregation*, this mechanical effect, which is so intimately connected with the thermic effect, appears in like manner whenever that is manifested, namely, *on the anode*, and it depends upon the positive polarity.

25. The arts have already derived advantage from this fact. Pring succeeded in making engravings by the aid of magnetic electricity, on a plate of tempered steel. With this view, he placed the plate in communication with the positive pole of a voltaic pile; he introduced in the circuit a wire helix; he operated upon the plate with a platinum point in connection with the negative pole; at each interruption of the contact of the point with the plate, particles of steel were detached from the latter; when the poles were inverted, there was, on the contrary, deposited at each spark particles of the needle on the steel plate.

26. This phenomenon of pulverization and incandescence is seen, in a still more distinct manner, and on a larger scale, in the arc of fire established at the positive pole between pieces of charcoal serving as electrodes. Ever since Davy, with his colossal battery of 1250 pairs, produced such an arc of four inches in length in the open air, and six inches in rarified air, reiterated experiments have confirmed, even on a less considerable scale, that this phenomenon is accompanied with the highest degree of light and heat that our instruments can produce. It has been constantly remarked that the carbon of the positive pole is reduced in a state of powder at its extreme point, and that the particles of carbon in a state of incandescence are carried away with the current toward the negative pole, so that on the anode there is formed a cavity, and on the

* *Pogg. Annalen*, vol. lxv. p. 181.

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cathode, on the contrary, a kind of excrescence, arising from an accumulation of particles of carbon. The incandescence and the secondary light attain on the anode the maximum of extent and intensity.*

It hence follows that this beautiful experiment is of little utility for the study of polar phenomena. In this respect the experiments with solid metals, such as those of Walker that I have just mentioned (20), are much more instructive.

28. If positive electricity has the property of reducing into dust and obliterating bodies, *negative electricity tends to give a form to the assemblage of particles, and to condense them.* Amorphous carbon, reduced to powder, and heated to ardent redness on the anode, when transported to the cathode is condensed then into a graphite, with a superior specific gravity, and which has a tendency to crystallization. The species of arborescent carbon which form upon the cathode of energetic batteries, by the effect of the passage of the incandescent current, belong to the same class of facts. It is perhaps the same for the light of crystallization, which is peculiar to different bodies, and which seems to be a kind of symptom of negative electricity. In the hydro-electric pole, also, the state of aggregation of the metal is destroyed on the anode by oxidation, and re-established on the cathode, where it takes the form of crystalline vegetation. The positive electricity, with the power it possesses of heating and dividing, attacks the conducting body in its intimate substance; negative electricity does not produce light, and only determines forms on the surface of this body.

29. To complete the demonstration of the contrast that we have just remarked, experimental researches have still to resolve the following questions:—Is the origin of the calorific power so restricted to the positive pole, that every elevation of temperature at the negative pole arises from the former? And, if the facts that have just been related give a great probability to this hypothesis, cannot we make one step more, and suppose that there is in the function of negative electricity a force the effect of which is to lower the temperature? In consequence of the reaction due to the positive heat, there is no doubt that this force could only manifest itself in a transitory and very insensible manner; but since Peltier's sagacity has succeeded in demonstrating the existence of this trace of cooling, at the point of contact of thermo-electric metals through which this philosopher passed an electric

* Fizeau and Foucault, in the *Annales de Chem. et Phys.* Series III. vol. ii. *Pogg. Annalen*, vol. lxiii. p. 169.

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current in a certain direction, we may hope also to obtain the same success in relation to the object we have in hand.

III. *On the Polarity of Light and Heat.*

30. To discover light without heat, and heat without light, in electrical phenomena is the ideal problem that the science proposes. But we can only reply experimentally by operating on the first development of electricity, or the activity which it has in a nascent state. If the electric development acquires energy, the heat resulting from incandescence produces on the anode itself a secondary light, and the transmission of heat makes the luminous cathode participate in the phenomenon without heat. Thus it is that we have met with phenomena in which it was a difficult matter to distinguish very clearly what belonged to light from what belonged to heat; like as it was not easy to recognise the part played by the two principles to the production of these two orders of effects. But, in studying the calorific effects, I have seen phenomena relating to negative electricity explained by aid of those connected with positive electricity. I should, therefore, again have to treat different points relating to the same subject.

31. Another object that must be followed out in science consists in seeking the common point by which light and heat are united. Every polar natural force is identical to itself, and is at the same time different from another. One of the directions, according to which its activity is exercised, is never manifested to us under the other. They cannot separately be made manifest; they do not exist isolated from each other. We must carefully avoid confounding with this simple point of view the complicated phenomena which are directly presented to us, and which are the object of our study.

32. It is not easy to find in electrical researches, a starting point to arrive at the solution of the second problem that we have proposed to ourselves; thus it is necessary to recur to an analogous phenomenon, the prismatic spectrum. In this latter, namely in light itself, there are luminous and calorific rays distinct from each other; and, as in the other case, light is also developed in the calorific phenomenon. The phosphorescence of phosphoric stones is excited by the blue calorific rays; and this light disappears under the influence of the red luminous rays.* Now the theory of undulations explains this striking difference in showing a simple difference in the length of the waves.

33. The phenomena in which the most remarkable mixture of light and heat is presented, is the *flame due to electric incan-*

* *Pogg. Annalen*, vol. lxiv. p. 334.

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descence. A flame is gas in combustion, according to the ordinary definition; and in all the modes of developing electricity abundant examples are found of flames of this kind. In fact, when the electric discharge acts with great energy, not only does it reduce the metal into a powder formed of incandescent particles, named sparks, but it also transforms the metal itself into vapour by a change in its state of aggregation, and the new vapour burns in the form of flame. Such is the mode of action of the Leyden jar, of the voltaic pile, as well as of magnetic electricity, when it is produced by a strong battery. By the aid of my magnetic electromotor, set into action by six pairs of Groves, I obtained violent combustions of the metals, which inflamed when it was brought near. The following is Söhr's description of the phenomena of combustion that he obtained with his machine, composed of three steel magnets.—"If we connect with the commutator the extremities of spirals placed one after the other, so that their currents are added; steel placed on steel gives sparks of a long roundish form, the centre of which, of a very dazzling bluish-white, is surrounded with a yellowish envelope, itself covered by a reddish envelope. In this case, there is no combustion of particles of steel; but this phenomenon is immediately developed with much energy, and with a strong hissing accompanied with a jet of sparks at the moment when the commutator is made to communicate with each spiral separately, in order to increase the quantity of electricity. These sparks inflame alcohol." The luminous jet established between the charcoal poles of the electrodes is a flame of the same nature; it is the same as that produced in Walker's experiment, related above. (20)

34. But there are also *cold flames*, namely, gases that diffuse light with so little heat that they cannot inflame bodies when placed in contact with them. If phosphorus is rubbed very gently in the dark, it gives forth light, and liberates a luminous vapour. The heat of this flame is as feeble as its light; they may both acquire a much higher degree of intensity, but this is only when the phosphorus is inflamed, or is rubbed very briskly, that this slow combustion is replaced by the real and much more rapid combustion. The first phenomenon is primary light; it is not the effect of incandescence. An oxidation undoubtedly accompanies it, but it is not its product. It would be doing violence to common language to give the name of combustion to this oxidation. Furthermore, there are other examples of gaseous substances that also give polar light with a minimum of heat and oxidation.

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35. In like manner a cold flame constitutes the luminous envelope of the cathode in the fundamental phenomenon. (10) This flame has a very slight degree of thickness, it is true; but it is visible and appreciable. Its light is incontestibly due to electricity, but not its existence. It appears to me probable that every metal in its solid state is surrounded by a gaseous atmosphere forming a very thin film, and that in this stratum resides the specific odour of divers metals; it differs from metal only in the state of aggregation. But electricity, the influence of which renders this film luminous and visible when it acts with the greatest intensity, may itself be so feeble as not to be able to produce an evaporation of this kind, either by the heat that accompanies it, or by the destruction of cohesion which is a consequence of its presence.

36. The presence of this metallic atmosphere explains another particular phenomenon. Indeed, besides platinum and steel, the coloured lights of which I have described above, I submitted zinc and copper to the same experiment. The first gives a blue light, but it also communicates a blue colour to platinum, when one is made to oscillate against the other; and the latter becomes luminous by serving as a negative pole. Copper gives a green light; it also transmits it to platinum, exactly in the same manner as steel does.

37. With regard to the light of points, it is incontestibly the metal itself, its solid surface, which is the luminous substance. In all the phenomena, there can be no question about electro-chemical decomposition, either of the aqueous vapour in suspension in the air, or of the air itself; these two substances are too bad conductors to be able to become luminous by induction under the influence of an electricity so feeble as that which produces light in the experiment in question. The phenomenon of primary light is manifested at the negative pole, is as independent and primary as the liberator of the gases is at the two poles in the decomposition of water.

38. It is quite otherwise with the light presented by electricity of friction. This electricity is of so high a degree of tension, that it electrizes by induction the worst conductor. If, then, when the electrical pencil is produced, the light is predominant at the positive electrode, it is the air electrized negatively that is luminous. Thus the light extends with the air when the experiment is made in rarefied air; this equally explains why different gases produce a light of a different colour.

39. I have also obtained in rarefied air, the luminous phenomenon with the magnetic electromotor. The violet

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flame attains a size six or eight times superior to that which it formerly had, and the luminous white points lose a portion of their brilliancy. Thus, the resistance to conductibility of the metallic surface, diminishes by the effect of the rarefaction of the insulating air, and the same thing occurs with regard to the metallic atmosphere.

40. I again repeated the same experiment in dry carbonic acid gas. A flame of a milk-white colour is obtained. We might believe that it is due to a very compacted agglomeration of luminous points, but it passes beyond the electrodes, whether the latter present a plain surface as for a conical form. Its size is considerably inferior to what it is in atmospheric air: the resistance of conductibility is therefore augmented. In the two latter experiments, the lens cannot be made use of on account of the bell-glass.

41. If it is true that negative electricity produces light and positive heat, we might inquire, if light and heat have altogether an electric origin. To answer affirmatively to this question would be to commit the same error as we see everywhere in chemical and magnetic phenomena, the simple effect of an electric current. There exist between the great forces of the universe an intimate affinity and reciprocal action; they are only separated from each other by limits that are very nearly approximated; but, at the same time, they are individualized and acquire an independent existence. It is not an inflexible law which connects them together, so that each link of the chain still holds to a superior link; but they are subordinate to each other in an active manner, and in a relation that is not always the same. The poles of a battery employed for chemical decomposition make manifest the acid and the basic properties, but these qualities are equally isolated and realized in material substances. In like manner, the development of magnetism, which always accompanies in a transverse direction the electric spark and current, is incorporated with matter by a coercitative force permanent in the magnet. Even when the magnet becomes such by electricity, it loses under the new form that it assumes the character of its origin, and it is only when its magnetic property increases in intensity or is destroyed, that it regains this character. Light and heat are in like manner sometimes connected with electrical phenomena, and at other times are independent of them, have a separate existence and react upon each other. As poles cannot act one without the other, so light and heat are reciprocally produced, although they have also, under other circumstances, a separate existence and are independent of each other. In the

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series of fundamental facts by which these reciprocal relations are represented, there is only one now wanting to science, namely, that which represents the phenomenon of subversion,—the production of electric polarity by light. This phenomenon would make known a limit beyond which science has not yet penetrated; but nothing announces that, in this hitherto unexplored field, an opposition may be met to the law which reigns over that which we already know. Though this blank of which I have just spoken is not to be essentially connected with the consideration contained in this memoir, it yet appeared to me essential to name it here, in order to take the opportunity of encouraging philosophers to make it the object of experimental researches.

42. I think I have demonstrated the following different points:—there is something inaccurate in the ordinary opinion, according to which electrical light is due to a spark passing from the positive to the negative pole: this light always arises at the negative pole, it is its original function; it is then a primary light, independent of heat; the origin of heat and the destruction of cohesion are the property of the positive pole; this property, by acquiring a superior degree of intensity, produces incandescence, then developes secondary light, transports to the negative pole particles of the anode, and even attacks the latter; by the effect of calorific transmission, so that, in the production of the phenomenon of incandescence, heat and light are confounded not in their origin, but only when they attain a superior degree of intensity. Always to see in heat the cause of light, and in the function fulfilled by the positive pole the cause of that fulfilled by the negative, is a theory as little accurate as that which attributes the origin of the electricity of contact to the part played by the positive metal. In both cases, there is an activity perfectly equal on the part of the two factors, concurring in the production of the action.—[A.]

Francfort sur Maine, 30th August, 1845.

CLXIII. *On the Direct Relation existing between the Colour of Bodies, and their Action upon the Galvanometer.* By Thos. Pollock, Esq. Communicated by the Author.

The following course of experiments was carried out by my friends Messrs. Mackrell, J. Gann, and myself. Our object was to determine if a relation existed between the change of

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colour induced in a liquid by heat; and of the deflection east or west of the galvanometer thus occurring.

To my friend, Mr. Mackrell, is due the merit of first observing, from the results presented by a former course of experiments, that the above relation existed. When the change of colour was from red to green, or from green to red, the galvanometer, by its opposite deflection, he observed, would attest the fact. To his ingenuity is due the means of meeting the experimental difficulties presenting themselves during the course.

To render the object of these experiments intelligible, a short historical review will be advisable.

On May 5th, 1838, a paper was read before the Electrical Society, and published in abstract in their "Transactions," entitled "On the Connection between the Atomic Arrangement and the Conducting Power in Bodies." The object of this paper was to show that transparent bodies might undergo change of temperature without a change in atomic arrangement; but that coloured bodies could not do this. The former atomic arrangement I proposed to call oscillating, the latter, reactive. It was then next insisted upon, that during the undulation essential to the progress of light, a change of capacity equivalent to change of temperature within the local influence of such undulation must of necessity occur, atomic change take place, undulations of different sizes be generated, and by whose mutual interference the light would more or less be destroyed. This, it was then stated, must occur in coloured bodies. It was lastly stated that transparent bodies, being less liable to atomic change, were more strictly allied to non-conductors; but coloured bodies, being more liable to this change, to conductors.

To M. Schoenbein is due the honour of proving that to a very considerable extent this distinction is correct.* He employed in a U tube different liquids capable of undergoing change of colour by change of temperature, such as nitrous acid and chloride of cobalt. On connecting the liquid in each arm of the tube by means of two pieces of platina wire with a delicate galvanometer, and applying heat to one arm, deflection of the needle took place, thus establishing conduction. But with no transparent liquid, except chemical action were known to occur, could he obtain this same action upon the galvanometer. It is to be regretted that this distinguished philosopher should not have further pursued this important and interesting inquiry.

On April 20th, 1841, a paper entitled "An Account of Experiments undertaken to investigate the Nature of the

* See Sturgeon's Annals of Electricity. Page 224, vol. v.

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Change of Colour in Bodies by Heat, and their Conducting Power,"* was read before the Electrical Society. Here the experiments of M. Schoenbein were repeated and varied. It was here shown that action upon the galvanometer could be produced during dilution of a coloured liquid with water; all which tended to show the intimate connection between colour and conduction. Mr. Mackrell, by comparing the results of this course of experiments, was led to suspect the existence of that relation between colour and the galvanometer, which it is the purpose of the present inquiry to elucidate.

Having occasion lately to examine the chloride of cobalt, the same as employed in the above experiments, it was found that, on the application of heat, it no longer changed to green, but to a bright red, and also the direction of deflection reversed.

This extraordinary change determined us to investigate whether, by placing different coloured liquids—as, for instance, green and red—in contact with each other in a U tube, and connecting them by means of platina with the galvanometer, the deflection of the needle east or west would bear any relation to the relative positions of such colours in the solar spectrum. To this inquiry the following experiments will be found to give a satisfactory answer.

Apparatus employed.—An astatic galvanometer, the same as employed in the former experiments: the needles suspended by a film of silk, and enclosed in a tall bell glass to secure it from the action of currents of air. Connected with the galvanometer were two copper wires thirty inches long, extending to a perpendicular piece of wood, in which were two mercury cups. By the side of the wood was placed the U tube, having the tops of its arms opposite the two mercury cups. The tube was about seven inches high; the internal diameter of the tube about $\frac{3}{8}$ths of an inch. It was placed upon a wooden upright stand. The tube thus stood with its arms west and east. The former designated by *a*, and the latter by *b*. We may here observe, that since the former experiments the polarity of the needles had been reversed.

Manner of performing the Experiments. — The heaviest of the two liquids was poured into the tube, nearly filling it to one-half. The other was then poured in carefully, disturbing the former one as little as possible, until it had displaced it to the bend at the bottom of the tube. The liquid having the less refrangible colour, as red, was placed in arm *a*, the more refrangible, as green, in *b*. The slips of platina were then inserted into each arm, and connected with the mercury cup, and the deflection of the

* See Proceedings of the Electrical Society, p. 9.

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needle observed. In preparing the liquids, no particular attention was paid as to purity. Thus the chloride of nickel was prepared from the common brown oxide and common hydrochloric acid; the chloride of cobalt from the common oxide.

In all the following arrangements the deflection was eastward.

Arm *a*. Colour less refrangible.	Arm *b*. Colour more refrangible.
1. Diluted sulphuric acid with tincture of cochineal.	Solution of potash with tincture of cochineal
2. Diluted sulphuric acid with tincture of Brazil.	Solution of potash with tincture of Brazil.
3. Solution of potash with a few drops of tincture of cochineal.	Solution of potash with double the quantity of tincture of cochineal.
4. Bichromate of potash.	Chromate of potash.
5. Chloride of cobalt, old.	Acid solution.
6. Chloride of cobalt, new.	Acid solution.
7. Chloride of cobalt, new.	Chloride of coblat, old.
8. Chloride of cobalt, new.	Chloride of cobalt, Zaffre.
9. Chloride of cobalt.	Chloride of copper.
10. Chloride of cobalt.	Chloride of nickel.
11. Chloride of cobalt.	Chloride of manganese.
12. Chloride of iron.	Chloride of cobalt.
12. Chloride of iron.	Chloride of copper.
14. Chloride of iron.	Chloride of manganese.
15. Chloride of iron.	Chloride of nickel.
16. Chloride of manganese.	Chloride of copper.
17. Nitrous acid, *red*.	Nitrous acid, *green*.
18. Nitrate of cobalt.	Nitrate of copper.
19. Nitrate of iron.	Nitrate of copper.
20. Nitrate of nickel.	Nitrate of copper.
21. Sulphate of cobalt.	Sulphate of copper.
22. Sulphate of cobalt.	Sulphate of iron.
23. Sulphate of cobalt.	Sulphate of manganese.
24. Sulphate of cobalt.	Sulphate of nickel.
25. Acid sulphate of iron.	Sulphate of iron.
26. Sulphate of iron.	Sulphate of nickel.
27. Sulphate of manganese.	Sulphate of copper.
28. Sulphate of manganese.	Sulphate of iron.
29. Sulphate of manganese.	Sulphate of nickel.

All the above arrangements in the deflection produced, agree with that attending chloride of cobalt, when heat is

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applied to arm *b*, that is eastward, and the current in the circuit the same; from the most to the least refrangible liquid in the tube, and from the latter to the former, through the wire of the galvanometer.

The following arrangements give a deflection to the westward. As they appear to constitute exceptions to the assumed general law, the results of the experiments are given more in detail.

Arm *a*.	Arm *b*.
30. Chloride of cobalt from Zaffre.	Acid solution, deflection, W.
Repeated.	No action.
Electrode *a* washed in distilled water.	Deflection, W.
Repeated.	Oscillation only.
Electrode *a* washed repeatedly in distilled water, and tested with ferrocyanide of potassium changed blue.	
31. Chloride of copper.	Chloride of nickel, deflection, W.
Arrangement kept up for 1½ hour.	No action.
Electrode *a* washed.	Deflection, W.
Repeated.	No action.
Repeated several times with the same results.	
32. Chloride of manganese.	Chloride of nickel, deflection, W.
Repeated.	„ W.
In 1 hour.	No action.
Electrode *a* cleaned.	Deflection, W.
Repeated in a few minutes.	No action.
33. Nitrate of iron.	Nitrate of nickel, deflection, W.
Repeated.	No action.
Electrode *a* cleaned.	Deflection, W.
Repeated.	No action.
Electrode *a* cleaned.	No action.

This arrangement was kept up some days. The colour of the iron solution had changed to a green from its original yellow. Electrode *a* marked with brown spots, which, when tested, gave a blue colour. Solution of ammonia in excess gave no indication of nickel.

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Arm *a*.	Arm *b*.
34. Sulphate of iron.	Sulphate of copper, deflection, W.
Repeated.	Less than before, W.
Electrode *a* tested.	Change to blue.
Electrode *b* tested.	No change.
35. Sulphate of nickel.	Sulphate of copper, deflection, W.
Repeated.	„ E.
Repeated.	„ E.
Electrode *a* tested.	Blue colour.
Repeated.	„ E.
The arrangement having been kept connected some time, on breaking contact.	Deflection, W.
The same occurred again after 8 hours.	Deflection, W.
On making contact.	„ E.

The colour of the sulphate of nickel changed from a yellow green to a most beautiful green.

Observations on the above Experiments.

They prove conclusively the existence of a direct relation between the colour of bodies and their action upon the galvanometer. They prove that the westerly deflection which apparently is at variance with it, is, on the other hand, a proof of its existence.

Experiment 1.—The action of the acid and potash upon the galvanometer is very slight; but when coloured very considerable; although twenty drops only had been added to six drachms of each. The deflection immediately recommenced on raising or lowering either of the electrodes. This property existed for some days.

Experiment 3.—Solution of potash alone gives oscillation, but no deflection; but when colour in different proportions is communicated to each arm, the deflection is great.

Experiment 5.—The acid solution is the same acid and water as is employed in preparing the chloride of cobalt. It therefore differs from that compound only in containing no cobalt. It is, in fact, a solution of chlorine, possessing a yellowish-green colour.

Experiment 7.—New chloride of cobalt is of a bright red colour; the old of a brown red.

Experiment 17.—These two acids give about the strongest deflection in the list.

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Experiment 30.—Here the deflection is only temporary, as is the case with all arrangements producing westerly deflection. This distinguishes them in the most marked manner from those giving an easterly deflection, many of which maintain this action for days together. The cause of the action being temporary, depends upon a larger and non-conducting matter; in this case, oxide of iron being deposited upon electrode *a*. This breaks the circuit: the equilibrium previously disturbed is now restored; hence the return current, with its momentary power, producing the westerly deflection, identical, both as respects cause and duration, with the return current of the secondary wire. None of the experiments in the most remote degree favour the idea of the existence of a current having a permanent action, and westerly, like that producing the easterly deflection.

Experiments 31 and 32.—Both prove the temporary action producing the westerly deflection.

Experiment 33.—This is a beautiful experiment: the iron solution changes its yellow colour to a beautiful green, thus retaining its protoxide, and throwing down its peroxide in a visible form upon the electrode. Hence the break of the circuit, the return current, and the temporary westerly deflection.

Experiment 34.—Proves the temporary action producing the westerly deflection.

Experiment 35.—This is a splendid experiment. Its action commences with a temporary westerly deflection, but quickly changes to the one producing the permanent easterly deflection. This permanency is beautifully evident, on breaking contact of the wire when the deflection is west, thus proving that during the previous eight hours the easterly deflection had been steadily kept up. So much for the temporary nature of the former, and the permanency of the latter action. During this change of action, the colour of the nickel solution changes from a dull yellow green to a magnificent brilliant green.

Thus the whole course of these experiments tends to show that the easterly deflection depends upon a permanent current established from the liquid, having the more refrangible colour, to that possessing the less; and that the westerly deflection depends upon the cessation of that current: both great facts, apparently at variance, tending to the establishment of a grand and invariable law of nature. I shall not now attempt to apply the laws of Black to the explanation of these experiments. They will strictly do this, and also determine the powers of induction and conduction, both peculiar and relative.

THOMAS POLLOCK.

Aug. 31*st*, 1846.

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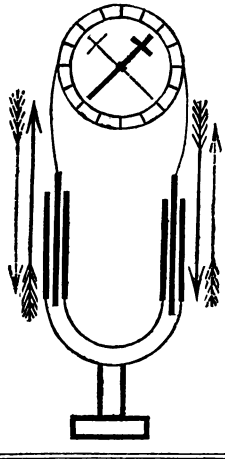

NOTE.—In the "Electrical Transactions," attached to the former paper, will be found a diagram illustrative of the arrangement employed. In this diagram, the black arrows denote the direction of the current connected with the easterly deflection: the dotted arrows denote the westerly.

CLXIV. *On the Polarity produced by Electric Discharge, and on its Employment for the Determination of the Quantity of ordinary Electricity associated with Bodies in combinations.* By M. BECQUEREL. From Comptes Rendus, March 9, 1846. Vol. xxii. No. 10.

(TRANSLATION.)

§ 1. HISTORY.

THE researches concerning the determination of the relative quantities of electricity associated with the elements of bodies in combinations, are of a high degree of interest to the physico-chemical sciences, on account of the relations by which the affinities are connected with electric forces. Mr. Faraday was the first to turn his attention to this question, but the process that he employed, although having given him a result approaching to the truth, does not permit of attaining the degree of precision necessary to induce us to place entire faith in it; and hence the importance which it deserved has not been attached to his work, nor to the theoretical views that he had deduced from it. Before setting forth the result of my own experiments relating to the same subject, I think I ought to give a succinct analysis of Mr. Faraday's work; which, at the same time, will be a means of showing the difference between his mode of experimenting and that which I adopted.

In a first memoir (*Philosophical Transactions*, 1833, p. 33) Mr. Faraday endeavoured to find a means by which he could compare and measure the electricity excited by our machines and that produced by the voltaic pile: with this intention he commenced by seeking whether the same quantity of absolute ordinary electricity, whatever were its intensity, passing

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through the wire of a galvanometer, under different circumstances, produced the same deviation of the needle. With this in view, he took a battery consisting of fifteen equal-sized jars, each (coated) eighteen inches high and twenty-three inches in circumference; consequently, each presenting a superficies of 184 sq. in., and the fifteen jars together a superficies of 2760 sq. in. The electrical machine that was employed to charge it had a plate fifty inches in diameter. The force of this machine was such that, when the plate was strongly excited, one turn was sufficient to give ten or twelve sparks from the conductors, each one inch long. All these numerical facts are indispensable to the solution of the question.

Seven pairs having been taken away, the remaining eight required forty turns to be completely charged; Faraday, however, charged thirty. He first made the discharge pass through the wire of a galvanometer, by placing in the circuit a moist string about 38 inches in length. The magnetic needle was deflected five divisions of his apparatus (arbitrary divisions). The seven other jars having been united to the battery, the latter was also charged by thirty turns, and the discharge made the magnetic needle deviate to the same amount. The experiment was repeated by carrying the charge to the maximum, and making the discharge pass sometimes through a simple moist thread, and sometimes through a slight cord moistened with distilled water, sometimes through a cord twelve times thicker and only twelve inches in length, moistened with acidulated water. With the thick cord, the discharge passed immediately; with the slight cord, it lasted an appreciable time, and with the thread, two or three seconds. In all these cases, although the current must have varied considerably in intensity, the deviation of the magnetic needle was always the same. Mr. Faraday hence concluded that, if the same absolute quantity of electricity passed through the galvanometer wire, whatever its intensity might be, the force of the current remained invariable. The deviation remaining constant, effectively only the same quantity of electricity must pass in the wire; but this fact does not in any degree prejudice the relation which must exist between this quantity and its intensity; for nothing proves that a portion of the electricity has not passed from one convolution to the other. It is probable, on the contrary, that, even though the convolutions had been separated from each other by a thin layer of gum-lac or a silk ribbon, the discharge in the different cases mentioned had such energy that it must without difficulty have broken through insulating matter opposed to its transverse passage. This lateral trans-

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mission of the discharge, if it had occurred as I suppose, must have exercised an influence over the result.

The battery of fifteen jars was charged by sixty turns of the machine instead of thirty, and discharged as before; the deviation of the needle was then doubled. Mr. Faraday deduced, as a consequence, that the force of the current is directly proportional to the quantity of electricity passing, whatever its intensity may be. The scale having been taken arbitrarily, nothing proves that the intensities were proportionate to the deviation; nevertheless, it is probable that he must have formed a table of intensities.

Mr. Faraday then arranged a voltaic apparatus, capable of producing an effect equal to that of the battery of fifteen jars; for this purpose he took a platinum point, and another of zinc, which he fixed in a parallel position, at a distance of five-sixteenths of an inch; two of their ends were placed in connection with the terminals of a galvanometer wire, and the other two were plunged into four ounces of distilled water containing one drop of strong sulphuric acid; the time was noticed which the needle required to oscillate on each side of its position of equilibrium. The zinc and platinum wires having been plunged five-eighths of an inch deep into the acid, and having remained there eight beats of a watch that gave one hundred and fifty beats per minute, were rapidly withdrawn. The needle continued to deviate until it had attained five divisions and a half, and it then returned to an equal distance on the other side. This experiment, several times repeated, having given the same result, Mr. Faraday inferred by approximation that two wires, one of platinum, the other of zinc, each one-eighteenth of an inch in diameter, and placed parallel at five-sixteenths of an inch apart, plunged in four ounces of distilled water, acidulated by a single drop of sulphuric acid, at the temperature of about 60°, and the free ends of which were connected with the extremities of a copper wire eighteen feet long and one-eighteenth of an inch in diameter; that these two wires, I say, gave as much electricity in $\frac{6}{150}$ths of a minute, as the electrical battery of fifteen jars, charged by thirty turns of a powerful machine in full activity. The magnetic needle having attained its maximum of evolution, was deviated five divisions and a third from its position of equilibrium. In this experiment, Mr. Faraday does not include the polarization of the pair, which necessarily diminished the intensity of the current—this pair not being constant.

Wishing, then, to compare the chemical action produced with the battery and the pile under the same circumstances, Mr. Faraday took a platinum wire one-twelfth of an inch in

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diameter, weighing 260 grains, and flattened at one of its extremities, so as to present a surface equal to that of its section, and placed it, in turns, in communication with the battery and with the voltaic apparatus, so that it was always the positive pole. This wire, being held in a vertical position, rested with all its weight on a band of test paper, placed on a platinum spatula communicating with the negative wire of the battery, or of the voltaic apparatus. This band was formed of four leaves of paper, moistened, at each experiment, to an equal degree with a solution of iodide of potassium. Ten turns of the electrical machine had a decomposing power, such that there was produced around the wire a pale spot of iodine; twenty turns, a darker mark; and thirty turns, a deep brown spot, that penetrated to the second leaf of the paper. The intensity of the chemical action, therefore, was determined by the number of leaves more or less stained. The wire and the spatula were placed together in the voltaic pair, in the circuit of which the galvanometer was included. This pair was made active with water acidulated by nitric acid, and was immersed in the liquid to a depth such that the magnetic needle was deviated at the first impulse five divisions and a third. The test paper was arranged and placed for experiment as before. The chemical effect of the current was observed, at several repetitions, for $\frac{8}{150}$ths of a minute, and it was constantly the same as that obtained with the electrical machine. This method of comparison only permits of our perceiving differences, for it is not susceptible of any precision. These data furnished Mr. Faraday with the means of also valuing, by approximation, the absolute quantities of electricity united with the particles or atoms in combination. (*Phil. Trans.* 1834, p. 77.)

The experiments of which I have just related the principal results, are of a nature *to give an almost overwhelming idea,* to use Faraday's expression, of the extraordinary quantity of electrical power proper to particles; in fact, when we think, adds he, of the quantity of electricity developed in about three seconds by such small particles of zinc wire in contact with water, that the loss of weight is scarcely appreciable by the most delicate apparatus, and without any trace of hydrogen being manifested on the platinum surface, we ask what an enormous quantity of electricity must be required to decompose a grain of water? According to Faraday's calculation, this quantity of acidulated water requires for its decomposition an electric current continued for $3\frac{3}{4}$ minutes, and capable of maintaining during this time, at a red temperature in the air,

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a platinum wire $\frac{1}{104}$th of an inch in diameter; the length of which he does not give.

Now, when we reflect on the excessive velocity of electricity, we may effectually conclude that the quantity of electricity that passes in this time must be excessive, and infinitely more considerable than that developed in the small voltaic pair previously described. Mr. Faraday having established a comparison between the two quantities, was led to this conclusion, that it appeared that 800,000 charges of the Leyden battery of fifteen jars would be indispensable to furnish the electricity necessary for the decomposition of a grain of water, or to equal the quantity of electricity naturally associated with the elements of this quantity of water, and in virtue of which they possess their mutual chemical affinity.

I have given some extent to this analysis, in order to show in what Faraday's method consisted. M. Peltier has also treated the same question (*Annales de Chim. et de Phys.* Second Series, vol. lxvii.), but with ideas restricted to the nature of electricity, and its relation to affinities; ideas which did not always permit of his regarding the phenomena under their true aspect. My intention is eventually to analyse this memoir. For the present I will confine myself to saying that, like Faraday, he transmitted common electricity through the circuit of a multiplier, without having taken the precaution to avoid loss. The results to which he arrived are more considerable than those obtained by Mr. Faraday; but as the means of comparison fail to value its unity, I will not relate them.

M. Pouillet also occupied himself in the determination of the quantity of electricity necessary for decomposing one gramme of water, but under another point of view, for he took as unity the quantity of electricity passing in a circuit of bismuth and copper, the total length of which is equivalent to 20 metres of copper wire, 1 m.m. in diameter, and the two solders of which have a difference of 100 degrees of temperature. In this determination there was no attempt at relation to a unity of ordinary electricity, but rather to a unity of dynamic electricity.

§ II. *On the Polarization of Gold and Platinum Plates by Ordinary Electricity.*

It is well known that when two homogeneous plates of an unoxidizable metal are placed in communication, each with one of the poles of a pile, and are plunged into a conducting liquid capable of being decomposed by the current, they experience such a modification that they are ready to produce

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a current moving in a contrary direction to the former, when the pile is taken away, and the circuit completed anew. This modification, which tends unceasingly to reduce the action of the primitive current, was first observed by Volta, and was immediately employed by him in the construction of secondary piles, which decompose water, act upon the electrometer, give sparks, and excite shocks in the frog. In a word, this modification constitutes what has since been called *electric polarization.* I would add, that my experiments have proved that this modification, this polarization, is due to the decomposition upon the decomposing plates of gases and other bodies transported by the current. These bodies, reacting on the liquid, produce a current determined in an opposite direction to the former. The only means of destroying the polarization is to remove the substances in proportion as they are decomposed. This is the principle that I have employed in the construction of apparatus with a constant current, which I have used for many years for the formation of insoluble crystalline compounds, analogous to those that are found in the divers formations of the globe,—a formation that requires a continuous action sensibly constant for months and years. A long time afterwards, constant batteries were constructed on the same principle, by giving them forms that rendered the employment of them easy.

The partisans of the constant doctrine saw, in the polarization of plates that had served for electro-chemical decomposition, nothing more than an antagonist electro-motive force, resulting from the accumulation of the gaseous principles of water on the decomposing plates; but the arguments which they set forth to support this opinion being of the same order as those which they employed to defend the contact theory, and which I have combated, I abstain from mentioning them here, in order that I may not recommence a debate which would be of no interest to science.

On the other hand, it has been asked whether the polarization acquired by two metal plates plunging in a liquid, and serving to transmit a current capable of decomposing it, was the result of an instantaneous discharge, or whether it did not require for its production that this action should be maintained for a certain finite time, although very short. In order to know upon what we are to depend in this respect, we must see what occurred in an instantaneous discharge, such as that of the Leyden jar. This has not been done, because we were far from supposing in this action a polarizing faculty so general, or so marked, as that which it does possess. It is precisely this faculty, of which I have studied all the consequences, and

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which has placed me in a condition to resolve, with a certain precision, the question of the quantity of electricity that is associated with the elements of matter, as at least that is necessary to separate these elements. The following is the general principle:—Every time that an ordinary electric discharge, however feeble it be, even that produced by a stick of gum-lac rubbed with a piece of cloth, traverses distilled water by means of two gold or platinum plates, these two plates are always polarized so as to give a current determined in a direction contrary to the primitive current; the more feeble the discharge is, the more sensible must the multiplier be with which the current of polarization is observed. The most feeble electric discharge that can be imagined cannot traverse a liquid without decomposing it, and consequently without drawing in its train material elements. I will give several proofs of this fundamental principle, in the course of this memoir. To go through experiments of this kind, we must have at our disposal:—

1st. An electrical machine, batteries, and coated plates of various sizes; 2nd, multipliers, sine-compasses of different degrees of sensibility; 3rd, several small flasks containing liquids into which the gold and platinum plates for polarization are plunged; 4th, a Lane's electrometer, giving with great accuracy the striking distance of the electric batteries. In the experiments that are to follow, we shall set out from this principle, that the explosive distance, the distance at which the discharge of a battery occurs, is proportional to the discharge, allowance being made for the residue of electricity adhering to the glass.

The apparatus is so arranged that the two gold plates plunging into the water are placed alternately in connection by means of a commutator, first with the coated surfaces of the battery, and then with the two ends of the wire of the multiplier, immediately after the discharge has taken place. The deviation of the magnetic needle indicates the direction and intensity of the secondary current, and consequently the intensity of polarization, but in certain cases only. Every precaution must be taken in order that the discharge passes entirely into the water; this is managed by insulating the various parts of the apparatus, with the exception, as a matter of course, of the outer coating of the battery. The commutator must be so arranged that the two circuits, that which served to polarize and that which gives the effect of polarization, are independent of each other, in order to be assured that all the discharge passes in the gold plate apparatus.

[The wires from the inner coating of the jar or plate have

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respectively two branches, one attached to a platinum plate in the liquid, and the other to a mercury cup: between the mercury cups and the galvanometer is the commutator, which, on being pressed down after the discharge, sends the current of the polarized plate through the galvanometer.]

The commutator must be manipulated rapidly, in order not to make a sensible diminution in the effects of polarization. The results are always comparable; seeing that the circuit which gives the current of polarization, and of which the multiplier consequently forms part, being always invariable, the conductibility does not change.

Finally, we must carefully form the table of intensities for each multiplier, which gives immediately the intensity of a current correspondent to a given deviation.

The series of experiments that follow were made with a coated plate, and a polarizing apparatus of the following dimensions:—

Coated Plate.

Dimension of glass plate.	Length	4·17	Dec.
	Breadth	4·17	,,
	Thickness	0·0025	,,
Coated surfaces	Length	3·19	,,
	Breadth	3·10	,,
Immersed parts of the gold plates	Length	3·5	Cent.
	Breadth	·7	,,
	Distance	4·0	,,

First Experiment.

Distance of the discharging ball from the coated surface, 2·256 m.m.

No. of discharges 1	10°·00	Deviation by first impulse.
	10 ·75	,, ,,
	10 ·25	,, ,,
	8 ·75	,, ,,
	10 ·00	,, ,,
Mean...	9 ·95	
Final deviation...	6 ·50	

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Distance of the ball from the coated surface, 4·51 m.m.

No. of discharges 1	21°·00	Deviation by first impulse.	
	21 ·50	,,	,,
	21 ·75	,,	,,
	21 ·75	,,	,,
	21 ·50	,,	,,
	21 ·00	,,	,,
	22 ·00	,,	,,
Mean..	21 ·25		
Final deviation ..	13 ·66		

Distance of the ball from the coated surface, 6·767 m.m.

No. of discharges 1	29°·25	Deviation by first impulse.	
	29 ·25	,,	,,
	30 ·00	,,	,,
	31 ·00	,,	,,
	30 ·00	,,	,,
	30 ·75	,,	,,
	30 ·00	,,	,,
Mean..	30 ·16		
Final deviation..	19 ·00		

Distance of the ball from surface, 9·023, m.m.

No. of discharges 1	35°·00	Deviation by first impulse.	
	35 ·00	,,	,,
	36 ·00	,,	,,
	35 ·50	,,	,,
	36 ·00	,,	,,
	37 ·00	,,	,,
	36 ·00	,,	,,
	36 ·00	,,	,,
Mean..	35 ·80		
Final deviation..	22 ·46		

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Distance of the ball from surface, 11·27 m.m.

No. of discharges 1.	43°·00	Deviation by first impulse.	
	44 ·00	,,	,,
	45 ·50	,,	,,
	43 ·00	,,	,,
	44 ·00	,,	,,
	44 ·00	,,	,,
	45 ·00	,,	,,
Mean..	44 ·04		
Final deviation..	26 ·30		

Distance of the ball from surface, 5·64 m.m.

No. of discharges 1	43°·50	Deviation by first impulse.	
	43 ·00	,,	,,
	44 ·00	,,	,,
	44 ·00	,,	,,
	43 ·00	,,	,,
	44 ·00	,,	,,
	43 ·00	,,	,,
Mean..	43 ·40		
Final deviation..	26 ·24		

Resumé.

Explosive Distance.	Deviation by first impulse.	Definitive deviation.	Intensity of current.	Calculated intensity.
1	9°·95	6°·50	6°·50	6°·30
2	21 ·25	13 ·16	13 ·16	12 ·60
3	30 ·16	19 ·00	19 ·00	18 ·90
4	35 ·80	22 ·46	23 ·70	25 ·20
5	44 ·04	26 ·30	30 ·60	31 ·50
2·50	43 ·86	26 ·24	30 ·52	

The results contained in this table show that for discharges of no great amount, such as those which may be obtained with a coated plate having the given dimensions, the polarization, to judge it by the current which it produces, is proportional to the explosive distance, and consequently to the quantity of electricity that passes in the water into which the gold plate was plunged, and that for a distance one half less and a double discharge, the effects are the same as for a double distance and a single discharge.

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These results are easy of interpretation—the polarization being due to the reaction upon the water, of the gases deposited upon the gold plates by the currents, must increase, at least the secondary current which is the consequence of it, until the surfaces of these plates are covered with it. A new addition of gas exercises no more influence over the intensity of the current, seeing that this is proportional to the extent of the active surface, and not to the thickness of the stratum. We understand, therefore, from this, that, beyond a certain intensity of discharge, the law formerly indicated can no longer be verified. Another observation: if we delay for a few instants after the discharge to make the current of polarization pass through the multipliers, the gases deposited upon the plates are gradually dissipated, and the current loses its intensity; so that we must operate with great rapidity if we wish to obtain very exact results. The differences between the intensities observed and the calculated intensities, which are not very considerable in the preceding table, must be attributed to this cause of errors, which we have always the power of reducing by manipulating the commutator rapidly. When once the polarizing current has traversed the wire of the galvanometer, and the needle has been deviated to a certain angle by the first impulse, it returns to zero after a few oscillations, so that there no longer remains any trace of polarization; nevertheless, the gases that are diffused in the liquid bring such a disturbance into the effects produced when we begin a new experiment a short time after, that regularity ceases to be observed. The results contained in the following table, and obtained by making the discharges succeed at intervals of a few minutes, after being assured that no traces of polarization remained, do not leave any doubt on this score:—

Battery of 4 *jars, each presenting a coated surface of* 1565 *c.m.*

Distance of the discharging ball, 2·256 m.m.

(The same gold plate as in the preceding experiment, plunging into distilled water to which several drops of nitric acid were added.)

No. of discharges 1	10°·00	Deviation by first impulse.	
	15 ·50	,,	,,
	16 ·50	,,	,,
	18 ·00	,,	,,

The results plainly show that the current of polarization increases in intensity for equal and successive discharges; whilst, if they occur at intervals a little distant, the intensities are perfectly equal, as it is easy to verify.

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I now pass to the effects of polarization produced upon two gold plates plunging into distilled water and serving to transmit a current of constant intensity, derived from one element of a pipe filled with zinc amalgam, plunged into distilled water, containing a platinum plate. The polarizing action being no longer instantaneous, as in the case of the discharge of an electric battery, a portion of the polarization must disappear when the voltaic circuit remains closed for a certain time, as we shall see :—

Duration of the current.	Deviation by first Impulse.		Final deviation.	Intensity of the current.	Difference.
1	21°				
	20	20°·33	11°·14	11°·14	
	20				4°·46
2	27				
	27	27 ·33	15 ·60	15 ·60	
	28				
3	33				
	33	33 ·00	18 ·89	18 ·89	3 ·29
	33				
4	39				
	37	38 ·33	21 ·91	23 ·10	4 ·21
	39				
5	42				
	41 ·50				
	41 ·50	41 ·66	23 ·86	26 ·67	2 ·37
	43				
6	44				
	43	43 ·33	24 ·81	28 ·00	1 ·53
	45				
7	44				
	45	44 ·66	25 ·75	29 ·50	1 ·50
	45 ·50				
8	45 ·50				
	46 ·00	45 .66	26 ·50	30 ·80	1 ·30
	47 ·50				
9	47 ·33				
	47 ·00	47 ·27	27·37	32 ·30	1 ·50
	47 ·50				
10	47 ·50				
	47 ·50	47 ·50	27 ·50	32 ·60	0 ·30
	47 ·50				

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In another series of experiments I found:

Duration of polar current.	Deviation by first impulse.	Final deviation.	Intensity of current.	Difference.
1″	27°·00	14°·50	14°.50	5°·50
5	39 ·50	20 ·00	20 ·00	
10	44 ·50	22 ·33	24 ·10	4 ·10
15	47 ·00	23 ·53	25 ·90	1 ·80
20	49 ·50	24 ·86	28 ·00	2 ·10
25	51 ·00	25 ·66	29 ·50	1 ·50
30	52 ·33	26 ·00	30 ·00	0 ·50
35	53 ·00	26 ·25	30 ·40	0 ·40
40	54 ·00	26 ·90	31 ·53	0 ·93
45	54 ·50	27 ·40	32 ·30	0 ·73

The results contained in these two tables suffice to show that polarization, measured by the current that it produces, is not proportional to the time during which the voltaic current circulates, as might have been expected, seeing that the gases or other substances deposited upon the decomposing plates are gradually separated, in order to mix or to combine with the water, and this in greater quantity according as the action of the current is more considerable, and as the action of the current has lasted longer; thus we see, in the last column, that the differences become the less as the electro-chemical action has been the more prolonged, so that they finish by becoming barely sensible. But it is not the same when operating with very feeble currents for a short time, such as the fraction of a second; we then recover the law formerly found, with ordinary electric discharges. The following results leave no doubt upon this head:—

I took a voltaic pair composed of a pipe head filled with mercury containing a very small quantity of zinc, distilled water, and two platinum wires, one plunging into the amalgam, and the other into the water of the polarizing apparatus. The following results were obtained:—

Duration of the polarizing current.	Deviation from first impulse resulting from the polarizing current.
1″·00	11°·00
0 ·50	5 ·00
1 ·00	10 ·50
0 ·50	6 ·00
1 ·00	11 ·00
0 ·50	5 ·00

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Mean deviation for a duration of 1″ ....10°·73
„ „ „ 8 ·5.... 5 ·33

The law is, therefore, verified in the circumstances under which we have been operating, namely, by acting with a feeble current operating for a short time. We must therefore arrive at a similar result with currents that are a little stronger, provided always that the circuit remains closed for a third or a quarter of a second. This effect is very readily explained; every time that the gases are deposited in a very feeble quantity on the gold plate submitted to voltaic action, they require a certain definite time, although excessively short, in order to spread in the surrounding liquid; by allowing, therefore, the current to circulate only for fractions of a second, we approach more and more the limits at which their liberation no longer occurs.

§ III.— *Determination of the quantity of Electricity associated with the elements of bodies in combinations; or at least, that is necessary to separate these elements.*

To arrive at this determination, I took advantage of the phenomena of polarization produced by electrical discharge upon plates of gold plunged into distilled water, and which permit of our making this comparison.

To arrive at this, we must commence by comparing together the effects of polarization obtained with ordinary or static electricity, and dynamic electricity.

First Experiment.—Description of the Apparatus.

(So far as the coated Leyden plate is concerned, the arrangement is similar to the last; excepting that when the commutator is turned, the current passes through a sine-compass and a galvanometer.)

The explosive distance being 4·51 m.m., the battery was charged until the discharge occurred; immediately afterwards the commutator was moved, so as to pass the currents resulting from the polarization into the sine-compass and the multiplier. In the latter, we had

Deviation by first impulse. { 1st, 31°·50 / 2nd, 31 ·50 / 3rd, 31 ·50 } 17°·97 final deviation.

Such is the effect produced in the multiplier by the single effect of the discharge of the battery. To obtain that resulting from the voltaic action, we introduce into the part of the circuit in which the polarizing apparatus is included, and of which the battery no longer formed part, a voltaic pair, that called *pipe head*, similar to the one formerly described, and acting with distilled water only. The first part of the

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circuit—that containing the polarizing pair—remained closed successively for a second and a half second, in order to have the effects of polarization proportionate to the quantities of electricity in motion. The following results were obtained with the multiplier:—

Duration of current.	Deviation by first impulse.	Mean.	Final deviation.
0″·5	17°·00 ; 16°·00 ; 15°·50 ; 16°·50	16°·25	8°·58
1 ·0	28 ·50 ; 29 ·00 ; 28 ·00	28 ·50	16 ·50

We see that the polarization is sensibly proportionate to the time, as might be expected, on account of the short duration of the circulation of the current. In the sine-compass, the deviation by first impulse was successively 2°·00, 1°·75, 1°·50; mean, 1°·75; corresponding to a final deviation of 0°·70. Now, we have seen that the discharge of the battery, when the explosive distance was 4·51 m.m., produced a polarization having as its measure a deviation of the magnetic needle equal to 17°·97. When we operate with the voltaic pair and the same circuit, in order that the effects be comparable, the polarization was 16°·50, the current having circulated for only a second. But, as the chemical action of the electricity and polarization, which is its consequence, are proportional to the quantities of electricity in motion, in the circumstances under which we are operating; and since, in this multiplier the intensities are proportional to the deviation up to 20°, it follows that we shall have the quantity of free electricity furnished by the current in a second, calling that furnished by the battery A *a*, by means of the following equation:—

$$17{\cdot}97 : 16{\cdot}50 :: a : x; \text{ whence } x = \frac{16{\cdot}50}{17{\cdot}97} = 0{\cdot}92\ a.$$

The quantity 0·92 *a*, has served me as a term of comparison in the following experiments.

The polarizing pair, as well as the multiplier, having been removed, I introduced into the circuit, 1st, a constant pair of copper and amalgamated zinc, acting with a saturated solution of sulphate of copper and a saturated solution of chloride of sodium, the latter contained in an unglazed porcelain diaphragm, plunged into the former solution; 2nd, a voltameter, composed of a very concentrated solution of copper and two plates of platinum. The circuit now consisted of only the sine-compass, the constant pair, the voltameter, and the connecting wires.

The pair having acted for an hour and a half, the experi-

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ment was stopped. The precipitated copper weighed 8·5 m.g., and the deviation of the magnetic needle of the sine-compass was constantly, throughout the experiment, 20°.25, corresponding to an intensity of current equal to 54. Now, in the preliminary experiment, the deviation of the sine-compass was 0°·70, corresponding to an intensity equal to 1·45. On the other hand, this current had allowed the passage for one second of 0·92 *a* of electricity (*a* being the charge of the battery as we have described).

The relation of the force being $\frac{54}{1{\cdot}45} = 37$, and the quantities of electricity that pass being proportional to the intensity of the current, it follows that the current that produced a deviation equal to 20°·25 would have allowed to pass in a second a quantity of electricity represented by

$$37 \times 0{\cdot}92\, a = 34{\cdot}04\, a.$$

On the other hand, in an hour and a half, namely, in 5400 seconds, we obtained 8 m.g. of copper, and consequently 8·00148 m.g. in one second. In order to obtain this latter quantity of copper, there must have been consumed, if I may so express myself, 34·04 *a* of electricity.

The quantity of electricity necessary for obtaining a milligramme of copper would, therefore, be given by the following proportion on account of the definite action of electricity:—

$$0{\cdot}00148 : 34{\cdot}04\ a :: 1 : x;\ \text{whence}\ x = 22980\ a.$$

In order to obtain a gramme of copper, it will be necessary to employ a quantity of electricity represented by 22980000 charges of the battery.

This experiment, on account of its importance, was repeated twice.

The current having circulated for 1 hour 40 minutes, or 6000 seconds gave 22·25 m.g. of copper. The deviation of the magnetic needle was constantly 37°. This deviation corresponded to an intensity of current equal to 135. The intensity of the primitive current, the deviation produced from which was 0°·70 was expressed by 1·45; the relation of the force, therefore, is

$$\frac{1{\cdot}35}{1{\cdot}45} = 93;$$

consequently, the principal current furnishes in one second $93{\cdot}00 \times 0{\cdot}92\, a$ or $85{\cdot}56\, a$ of electricity, the quantity of copper furnished by the current in one second being equal to

$$\frac{22{\cdot}25}{6000} = 0{\cdot}00370.$$

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0·00370 m.g. of copper, therefore, have required, in order to be obtained, 85,56 *a* of electricity.

For one milligramme, 23400 *a* would have been required, and for one gramme 23400000 *a*.

Finally, a third experiment gave the following result:—

Intensity of the producing current	137·95
Intensity of the primitive current	1·45
Relation of the current	91·00
Copper precipitated in one hour	13 m.g. 00
Copper precipitated in one seeond	·000361
Quantity of electricity formed in one second .	83·75 *a*
Quantity necessary to obtain one m.g. of copper	2320000 *a*
Quantity necessary to obtain one g. of copper .	23200000 *a*

To sum up, the three preceding experiments show that, in order to obtain one gramme of copper, it is necessary to employ the following quantities of free electricity:—

1st, 22980000
2nd, 23400000
3rd, 23200000

The mean is about 23200000 *a*.

Now the electric battery, which served as unity, consisted of four jars, each presenting an outer and an inner coated surface, each of 1565 sq. c.m.; consequently, the entire battery gave on each of the two faces a coated surface of 6260 sq. c.m.

If we refer the effect to a battery presenting a coated surface of one square metre or 10000 sq. c.m., we must then employ, to reduce a gramme of copper from a solution of nitrate or sulphate of copper, 14523200 charges.

This same charge will serve to obtain 0·25 grammes of oxygen, the atomic quantity proportional to 1 gramme of copper, by submitting to the action of the same current the solution of nitrate of copper and water contained in a voltameter, consequently, to decompose a gramme of water and to obtain 0·88 grammes of oxygen, we must set in motion 51586400 charges of the battery having a coated surface of a square metre, charges which are not nearly brought to their maximum, for the discharges were made when the ball of the discharger was 4·51 m.m. distant from one of the coated surfaces. The maximum discharge occurred when the explosive distance was about 11·28 m.m.; I say *about*, because I could only with difficulty obtain the discharge at this distance. In comparing the quantity of electricity with that which is furnished in the case of maximum charge, it is necessary that this charge be to 51586400 as 2 is to 5, which

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is the inverse proportion of the explosive distance, the quantities of electricity being proportional to the explosive distance, we therefore find for its value 20063456.

Faraday found, as we have seen in § 1, that to decompose one gramme (0·065 grs.) of water we must employ 800000 discharges of a battery, having a coated surface of 17760 sq. cent. or 1420800 charges of a coated surface of one square metre, electrized with a machine that I suppose was of the power of mine; therefore, in order to decompose one gramme, it would be necessary to put in motion 2185845 charges, whilst I have found by a method much more exact 20063456 charges at the maximum tension of a similar battery. The difference is not so great as might have been expected on account of the reason that I shall explain hereafter. I will again remark on this subject, that when a discharge of strong tension is transmitted in a wire that forms the circuit of a multiplier, as Faraday did, it is almost impossible to avoid a greater or less portion passing from one convolution to another, even though each of these convolutions be insulated, both by rolling the wire with silk, and by applying gum-lac as the wire was wound.

I still persist in believing that my method is susceptible of giving very accurate results when the multipliers have acquired a degree of precision which will permit of our estimating the intensities of the current to very nearly a small fraction of a degree; for we very readily perceive that when we act upon numbers so considerable as those which express the quantities of electricity necessary to decompose a gramme of water or of any other substance, an error of a quarter of a degree only in the value of the deviation of the magnetic needle brings a very notable difference in the value sought after. Whatever be the case, I think that 20000000 in round numbers, which expresses the number of charges serving to represent the quantity of electricity necessary to decompose a gramme of water is not far removed from the link. This number may be considered as a limit. It is frightful to the imagination to see that in order to decompose only a milligramme of water we require 20000 charges of a battery presenting a coated surface of one square metre, or, which comes to the same thing, the charge of a coated square having a superficies of about two hectares (5 acres).

The quantity of electricity associated with the oxygen and hydrogen in a milligramme of water only, and which represents, if I may so express myself, their reciprocal affinities would, therefore, be capable of producing the effect of thunder. All this electricity is in the state of equilibrium in bodies,

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and only becomes partially free in decompositions, because we have no means of avoiding the recompositions, to which may probably be attributed the effect of heat in chemical actions.

This enormous quantity of electricity which is enchained between the molecules of a milligramme of water, may serve to make us conceive how the electric fishes may at pleasure dispose of a considerable charge to give shocks.

It is sufficient, in fact, to admit further that these animals possess the property of decomposing an excessively small quantity of water, of appropriating each of the electricities that become free, and of preserving it in a particular organ to dispose of it at pleasure.

We may also explain, by the same reason, why a dry pile, when it contains only an excessively small quantity of water, may yet serve to charge a condenser abundantly.

Finally, it is now demonstrated that the constituent principle of a gramme of water contain between them an enormous physical power, capable of terrifying the imagination, and of which we ought to endeavour to obtain possession for the study of nature and the wants of society.

IV. *Another series of facts relative to Polarization, and the consequences that are deduced from them.*

It has been established incontestibly in the second paragraph, that the most feeble discharge of ordinary electricity, when transmitted through a liquid conductor by means of two gold plates plunged into this liquid, polarizes these plates, and that the effect is always appreciable when the multiplier is endowed with sufficient sensibility. This polarization can only occur in proportion as the water is decomposed. Thus electricity cannot travel in a liquid without drawing matter with it. Facts of another order than those formerly explained, will manifest this principle anew.

Thermo-electric currents derived from a single pair of antimony and bismuth, or iron and copper, scarcely deviate the magnetic needle of a multiplier for any part of the circuit in traversing distilled water; yet they are able to polarize the gold plate plunged into this water. Take an iron and a copper wire soldered by one of their extremities, and in communication by the other two with a very sensible long wire galvanometer, and let the polarizing apparatus be placed in the circuit; it is only necessary to elevate the temperature of the points of junction, iron and copper from 30° to 40° C., and to moisten the circuit closed for a few moments, in order to obtain a polarization current of at least 5°. The water, therefore, has been decomposed by the passage of the thermo-

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electric current, that is, by electricity whose tension is excessively feeble. By operating with an ordinary thermo-electric pile, composed of small elements, bismuth and antimony, furnished with conical reflectors, and by heating one of the faces by radiation from an ordinary lamp, with a double current of air, placed at a decimeter distance from this face, I obtained the following results, analogous to those which had been given to me by the voltaic pairs.

Duration of the passage of the thermo-electric polarizing current.	Deviation by first impulse produced by the polarization current.
1 minute	2°·00
2 ,,	2 ·50
5 ,,	2 ·75
10 ,,	3 ·75
20 ,,	3 ·75

We see by these results that, when the circuit traversed by a thermo-electric current is closed for ten minutes, the gases transported by this current upon gold plates, and which commenced to mix themselves, or to combine with the water when this current circulated for about a minute, attain, at least the deposited films, a maximum which we should in vain endeavour to overpass. By following the course previously marked out, nothing would be more easy than to determine the quantity of free electricity furnished by the thermo-electric or polarizing current, for the fraction of a second, and so comparing the voltaic and thermo-electric currents.

Since the most feeble quantities of electricity, when traversing a liquid conductor, react chemically upon it, so as to separate its elements, we might anticipate that the discharge of dry piles would also polarize the gold plates employed for transmitting their discharge through water. In fact, if we take a dry pile, composed of only a few hundred elements, and place the two extremities in communication with the two gold plates, a few minutes only are required to polarize these latter, so as to give a secondary current, causing the needle of a very sensible multiplier to deviate several degrees by the first impulse. This fact, therefore, proves that even although we cannot obtain any sensible effects of decomposition with a dry pile composed of a very inconsiderable number of elements, yet these effects are sufficient to produce polarization. These effects, although excessively feeble, by accumulating finally render the secondary current appreciable.

I have said in the second chapter of this memoir, that the electricity derived from the friction of a stick of gum-lac against a piece of woollen cloth in traversing water decomposed it so as to polarize the plates. The following is an

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experiment that proves this:—The pair of gold plates (the pair for polarizing) was placed on a plate of resin, so as to be perfectly insulated, and the two plates fixed by one of their extremities to a gold wire traversing a tube that passed into the cork of a bottle. This pair could be placed in connection with a very sensible multiplier, or else be detached from it in order to become polarized; in this latter case, one of the ends of the gold wire was held in the hand when the stick of electrized gum-lac was brought near the others; four or five minutes were sufficient to deviate the magnetic needle by first impulse from 0°·58 to 0°·75, when the communication was established with the multiplier. Had I had at my disposal a more sensible apparatus, it is very certain that a single contact would have sufficed to have enabled us to observe the current of polarization.

From this, we may say, without fear of being deceived, that an electric discharge, however feeble it may be, could not traverse water without decomposing it, and, consequently, without drawing in its course material elements.

If this fundamental fact be connected with Fusinieri's observations relative to the transport of imponderable particles by electric discharges of high tension, through air and metal, a transport which is brought about in such a manner that if the discharge occurred between a silver ball and a gold ball, silver is deposited upon the gold ball as well as in its mass; and reciprocally we may conclude from this fact and from those that precede, without having recourse to any hypothesis that electricity, or at least the principle that produce all these effects, could not exist when in motion, without being accompanied by matter; so that, in this respect, it is so identified with it that its existence is intimately connected with its own.

We are naturally led to examine a question which is not without interest for physiology in general; we know that there is always existing in the air a certain quantity of free electricity which is constantly positive in serene weather, and sometimes positive sometimes negative, under other circumstances; the earth being in an opposite electrical state, there must consequently be produced continual discharges by the intervention of the bodies that occur at the surface of the earth, such as iron, animal, vegetable, inorganic substances, &c.

These discharges must be accompanied by a succession of chemical decompositions and recompositions, both at the surface and in the interior of all bodies.

Mr. Faraday had already shown that if we place upon a

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band of very sensitive test-paper a metal wire in communication with the conductor of an electrical machine in action, and if we touch the same paper with a moist thread supported by a glass tube, there is an electro-chemical effect; the air is one of the decomposing surfaces, and the extremity of the wire the other.

The facts that I have just detailed in this memoir demonstrate, further, that this phenomenon occurs, however feeble the tension of the electricity may be, and under circumstances in which we could not suppose the electricity to have a decomposing action. We cannot, therefore, at the present day, doubt that the escape of atmospheric electricity through the bodies which cover the surface of the globe is accompanied by chemical actions that have a greater or a less influence over the phenomena of life in organized bodies, and on the composition of inorganic bodies. We are ignorant what these modifications may be; we only know that, when the electricity of the air is positive, the portion of air surrounding all bodies constitutes the positive pole, and the earth relative to the roots the negative pole. The chemical efforts must be dependent upon this electrical state.

Such are the consequences that flow from the researches in which I have now been occupied, on polarization by means of Free Electricity, and on the determination of the absolute quantity of ordinary electricity associated with the elements of bodies in combination. These developments will, I hope, serve to give more force to these consequences, which cannot fail of being interesting to natural philosophy.—[A.]

CLXV. *Phenomena of the Late Storms: extraordinary change produced in the Tone of a Clock-bell.* By J. Underwood.

To the Editor of the Electrical Magazine.

Sir, — The storm of Saturday, the 1st of August, for violence, I believe, stands conspicuous in the annals of meteorology. But there were other phenomena besides violence, each of which, doubtless, will be interesting and instructive to your readers.

The storm of Tuesday, the 4th, though less violent, produced similar phenomena. About an hour before the storm of Saturday began, while they were performing certain experiments upon some gases at Apothecaries' Hall, a change in

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their colour was observed; and upon examination, the atmosphere was found to be highly charged with electricity.

But the following circumstance is still more remarkable:—

A friend of mine, living in Camberwell-grove, Surrey, had a clock, the bell of which, until the storm of Saturday, was of a peculiarly beautiful tone; but the storm which then visited us stopped the clock and changed the tone of the bell to that of a low and disagreeable noise; but when the storm was over, its original tone was restored to it simply by shaking the clock and exposing the works to the fresh air, which now continued uniform until the storm of the following Tuesday, which had the same effect upon the bell as the first storm, without, however, stopping the clock; but the means which were originally effectual in restoring it to its proper note were now found to be useless. I have since examined the bell myself, but can find no crack or any symptom of one, which, previous to examination, I quite expected.

Mr. Weekes of Sandwich, with whom I have corresponded upon the subject, suggested the following means of explaining it, which as far as they go, I think are highly tenable; but they fail to show the cause of the clock stopping, or the reason that the same means which restored the bell after the first storm, were ineffectual in doing the same after the second; though on the latter occasion the storm was less severe than on the former. He says:—

"I believe that we are entitled from observations and experiments to conclude that the molecular particles of all metals when in a fluid or certain other conditions, in cooling down arrange themselves in an electro and polar direction, both as respect themselves and relatively to one another. My experiments and the researches of others also have shown that this molecular arrangement is capable of disturbance from the application of a new electric force artificially directed, as shown, for instance, in the production of musical notes and other (clicking) sounds which are obtained where a voltaic current is suddenly withdrawn from a wire through which such currents have been traversing for a few seconds. My inference, therefore, with diffidence suggested, is, that the molecular arrangement of your friend's clock-bell originally was such as was highly favourable to the production of agreeable sounds. During the late storms a current of atmospheric electricity must in some way have traversed the metal of the bell, and thus its molecular arrangement become changed. A blow, in the first instance, served to rectify what perhaps we may be allowed to call the error of the new arrangement; a second storm served to reproduce the pheno-

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mena of the first in the molecules of the bell; and it is probable that a few occasional strokes and a little time may suffice to restore the instrument to its original condition. Should your friend be anxious about restoring it, I would suggest to him to place the bell in an oven of somewhat high temperature for an hour or two, and then allow the heat to go down gradually."

Being lately much engaged, I have not had time to make any experiments myself relatively to the subject; but it has struck me, if we allow (which we certainly have great reason for supposing) that the attraction of cohesion and electric attraction are identical, or at any rate we have no hesitation in affirming that, if not identical, the attraction of cohesion is greatly modified by an electric current, in some cases being altogether annihilated, if I may use the expression, which, of course, I do not mean abstractedly; in others being greatly increased, and in others again, repulsive power seems to be given. I say, if we allow this, I think we may explain the phenomena in its general bearings; but I have not time now to show minutely in what way; and, as I shall not be able to experiment to any extent upon this subject for some months to come, I would throw the matter quite open to those who have more leisure, and are more accustomed to experimental philosophy than I am; and I shall be most happy if my suggestions shall have been the means of opening the path to new research. I am, sir, your obedient servant,

J. Underwood.

August 28th, 1846.

CLXVI. *On the great Thunder-Storm and extraordinary agitation of the Sea,* July 5, August 1, 1846. Read Aug. 11th, before the Penzance Natural History Society. By R. Edmonds, Jun.—Literary Gazette.

The extraordinary agitation of the sea in different parts of Mount's Bay, at the commencement of the great thunder-storm which passed over Britain on the 5th ult., was noticed at the last meeting of this society. I have now to notice a similar phenomenon observed at Penzance pier, early in the morning of the 1st instant, when a still more dreadful thunder-storm visited the metropolis and other parts of England.

I would previously, however, make a few remarks on the storm of July. It must have been felt on the Atlantic on the 4th, as much distant lightning from the S. or S.W. was

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seen in Mount's Bay before midnight, and continued until between 3 and 4 o'clock of the following morning, when the fierce lightning and thunder from every part of the heavens became truly alarming. As the storm proceeded from Mount's Bay throughout Cornwall, towards the N.E. and E. it grew more violent and destructive. It reached Exeter between 8 and 9 A.M.; Windsor, between 2 and 3 P.M., and London at half-past 3. In its progress towards the north of Britain, it was felt throughout Somersetshire between 8 and 9 A.M; at Leeds, about a quarter past 3 P.M; at Penrith, Dumfries, Ayr, and Glasgow, between 3 and 4 P.M.; at Edinburgh soon after 5; at Dundee and Argyleshire about 7; and Aberdeen shortly after midnight. Thus the storm, as it advanced from Mount's Bay towards the east, moved at a considerably slower rate than in its progress northward.

In many places, both the lightning and the thunder were continuous during most of the storm. A whirling, fitful, or "wild kind of wind" accompanied it, with heavy rain or large hailstones. At Ayr and Maybole it was preceded by a whirlwind of remarkable violence. At Walsall a whirlwind tore up trees by the roots. The temperature on the 5th July was unusually high, not only in most parts of England, but on the Continent. The thermometer at Frogmore, in the royal gardens, stood at 93° in the shade; at Manchester 87°; and at Paris, 97½°. Before the storm commenced in Kent, one of the largest flights of butterflies ever seen in this country completed its passage from France to England. For many hundred yards it quite obscured the sun. During the passage the weather was calm and sunny; but an hour or so after they reached *terra firma* it came on to blow great guns from S.W., the direction whence the insects came. The storm of July was not felt in London and the eastern coasts of Britain so severely as on the western. But the storm of August 1st, which visited the metropolis, and other parts of England, was more terrible and destructive than any experienced there since 1809. It commenced in Greenwich at 3h. 10m. P.M. In the Meteorological Report from the Observatory, it is stated that at Lewisham the hailstones were nearly all as large as pigeons' eggs. It was felt severely the same evening at East Walden, Leicester, and Nottingham; and before midnight, at Southampton and Paris. About 4 A.M. of this day, an extraordinary agitation of the sea was observed by the labourers employed in excavating Penzance pier, the tide being then about five hours' ebb, and the sea very calm. The water suddenly returned towards the shore, to the depth of between one and two feet, and after an apparent pause, rushed back "like a

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river," to its former level; the time occupied in the influx and the reflux, including the time of the water appearing stationary, was about six minutes. The flux and reflux were observed only once. My informant, who witnessed it, is the superintendent of the men engaged in the excavations

In London, the thermometer on this day was 89½° in the shade, and 116° in the sun, the latter being the maximum there for the year; and throughout the previous night it did not descend below 70°. At Paris it was 90° a great part of the day. At Penzance the nights of the 29th, 30th, and 31st, were the warmest hitherto of the year. Early in the morning of the 30th a terrific thunder-storm, with heavy rains and hail occurred in Mount's Bay and throughout Cornwall, and also in Wales and Cumberland, from which time, until after the great storm of the 1st of August, the atmosphere in Cornwall, London, and probably throughout England, was very sultry and highly charged with electricity, whilst violent thunder-storms were experienced in various places. An earthquake shock was felt along the Rhine in the evening of the 29th of July. The moon's first quarter was on the 31st, and the agitation of the sea above described (the result, perhaps of a submarine shock) happened on the 1st of August.

In conclusion I may remark that, with only one exception, all the shocks of the earth and extraordinary oscillations of the sea in Cornwall during the present century, whose dates are known, and which are eight in number, have happened nearer to the moon's first change or quarter than to any other. The shock not occurring at that period of the moon's age was felt in the eastern parts of Cornwall on the 20th October, 1837, the day before the moon's last quarter.

THE THUNDER-STORM OF SATURDAY, Aug. 1st, 1846.

From "The Times," *Monday, August 3rd.*

On Saturday, the metropolis and its suburbs for miles round were visited by one of the severest storms of thunder, lightning, and hail, accompanied by a heavy rain, that has been experienced for several years past. For three or four days previously the heat had been most intense, accompanied by a strong wind from the east, and a cloudless sky. On Friday afternoon, however, some clouds began to gather from the S.S.E., and as the evening advanced repeated flashes of vivid lightning broke forth, which continued throughout the night. Saturday morning broke very hazily, but as the forenoon advanced the mist cleared away, and between nine and ten o'clock the sun came forth with intense power, and from

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that period the heat continued to increase. After the haze had cleared away, the clouds which had presented themselves on the previous afternoon still appeared in the S.S.E., and although the wind remained in the N.E., they gradually extended themselves over the heavens, their increasing density too surely indicating a heavy thunder-storm. From two o'clock distant peals of thunder were distinctly heard, but the storm did not commence with any violence until about twenty minutes after three o'clock, when it burst over the metropolis and the western suburbs with exceeding violence. The lightning was remarkably vivid, and the flashes followed each other rapidly. The peals of thunder were also heard with great frequency; many of them resembled the sudden discharge of a large piece of ordnance, the reverberation of which was heard for several minutes. The rain was particularly heavy, and accompanied at intervals by hailstones of a very large size, many of those picked up weighing from an ounce to an ounce and a half. The storm was at its greatest fury between four and five o'clock, and lasted from twenty minutes past three o'clock until half-past six o'clock in the evening,—three hours and ten minutes.

The damage done by the storm has, no doubt, been most extensive, but its exact extent will not be ascertained for some days.

Amongst the public buildings in the metropolis which have suffered most severely by the fracture of skylights and domes, from the hailstones, are Buckingham Palace, Somerset House, Burlington-arcade, the Bazaar (Baker-street), the British Institution, Pall-mall, and the Paddington terminus of the Great Western Railway. The Horticultural Gardens (Chiswick), the Botanic Gardens (Kew), and those in the Regent's-park have also suffered severely.

At Buckingham Palace great damage was done. The picture-gallery, in which are some of the most splendid productions of the old masters, was at one time in imminent danger, for when the large hailstones, or rather irregularly shaped pieces of ice, had removed all opposition to the ingress of the rain, the gallery was flooded to the depth of several inches, and the water assumed all the appearance of a natural current. No time was lost in removing all those pictures that were liable to be injured, and we believe that in no single instance has damage been done to any one. The more valuable pictures were ranged under the engraved plate glass, which successfully withstood the violence of the storm.

As soon as the rain had in some degree abated, active measures were taken to clear the various rooms and lobbies of the palace from the water that had accumulated, and to achieve that object speedily, the services of the police stationed at the palace were called into requisition, and a company of the Foot Guards was sent from the Wellington Barracks to assist. Their services were put to a severe test for more than an hour. Mr. Cobbett, the painter and glazier to the Royal palaces, visited the palace, and having ascertained the extent of the damage, he at once gave the necessary

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directions for covering in the picture-gallery and the various rooms with tarpauling, and a great number of men were instantly set upon that work.

To state the amount of damage done to the palace would at the present moment be impossible. Many thousand squares of glass are demolished; and, at a rough calculation, the loss in that article alone will be little short of 1,800*l.* or 2,000*l.*

It is satisfactory to state that the Queen's decorators have closely inspected the various royal apartments into which the water obtained access, and have not discovered that any material damage has been sustained by any of the costly embellishments.

The parade in front of the palace, extending from the foot of Constitution-hill to James-street, Buckingham-gate, was one sheet of water, and so deep as to render it impassable to pedestrians.

The deep barrel drain in the Green-park burst from the immense accumulation of water, and formed a complete river to the ornamental water in the enclosure of St. James's-park. Here the violence of the storm dismantled the flower-beds, tore up the paths, and so flooded the pastures that it became necessary for men to wade up to the middle to rescue the sheep that would otherwise have been drowned. The water accumulated to such a depth in Bird-cage-walk that it was rendered impassable to pedestrians, and almost so to vehicles.

At the west end of town, in addition to the above, the premises of Messrs. Swan and Edgar, in Regent-circus, have suffered considerably from the effect of the hail. The extensive carriage-factory of Messrs. Marks and Co., nearly opposite the Polytechnic Institution, Regent-street, had the skylights completely destroyed; as had also Mr. Heyland, jeweller, Gerrard-street, Soho.

At the metropolitan police-office, Scotland yard, three hundred squares of glass were broken. The Admiralty stables sustained great damage, and not a house in Whitehall-place Gardens escaped. Lord Liverpool's and Sir W. James's windows suffered most.

We were also informed, from a credible source, that the Houses of Lords and Commons were so inundated, from the destruction of the fan-lights, that it may be questionable whether they will be in condition this evening to receive the members.

At the Houses of Parliament, fourteen men were engaged the whole of yesterday (Sunday) merely cutting out the pieces of old glass left in the windows that were broken by the violence of the storm. It is expected that the House of Lords will be repaired by ten o'clock this morning, and the Commons by noon. The total number of squares demolished in the two houses of Parliament and Westminster-hall is upwards of seven thousand. The law courts at Westminster were completely deluged with water, and the windows broken. At Westminster Abbey very little if any glass was broken, but the roof being under repair, the rain fell through in such bodies that several men were employed on Saturday night to bale it out. Upon inspecting the theatre of Westminster Hospital

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yesterday afternoon, the floor was found completely strewed with fragments of broken glass. The reading-room of the same institution had likewise a number of squares of glass demolished, and the room laid under water. The Westminster school of medicine in Dean-street has suffered most severely from the storm. At Lady Rose's mansion, in Old Palace-yard, a chimney-pot was struck down by the lightning, and nearly fell upon a man's head who was seeking shelter in one of the doorways. At Messrs. Cubitt's factory, Millbank, the damage done is considerable. It is stated that from twelve to fourteen thousand squares of glass are demolished, independent of the damage that must of necessity have been caused by the rain falling into the premises. Messrs. Broadwood's pianoforte manufactory in the Horseferry-road, Westminster, is likewise extensively damaged. One of the workmen informed the reporter that he believed nearly eight thousand squares of glass were broken.

Charlton-terrace has suffered very much, the vestibules of those houses which are glazed being considerably shattered.

Belgrave-square.—Scarcely a house in the square or its vicinity has escaped receiving some injury. One mansion alone has 70*l.* worth of glass demolished, and many of the rooms saturated with water.

The Regent's Quadrant.—Every square of glass over the Quadrant was demolished, and the pathway strewed with broken fragments. Several of the large houses have their stock in trade greatly injured.

In the neighbourhood of Brompton, Kensington, and Hammersmith the storm was particularly severe. About four o'clock, at the moment that a terrific peal of thunder burst over Kensington, the electric fluid struck a stack of chimneys at the house, No. 21, Lower Phillimore-place, at the corner of the road leading to Notting-hill, by which the stack has been much shattered, and several of the chimney-pots shivered to pieces. In the Victoria-road a skylight was carried by the force of the wind into the middle of the road. In the lower parts of Kensington and Brompton, the basement stories of several of the houses were inundated by the great body of water which descended like a torrent from Notting-hill and the higher parts of that locality, and which carried everything before it.

The nursery-grounds westward of town have suffered severely, as have also the market gardeners, their "lights" being all more or less damaged. The residence of Mr. Kelsell, Fair Lawn-house, Acton-green, has suffered severely, as has also the premises of Mr. Credland, and the house of Dr. Lindley, Acton-green. At Chiswick; the nursery of Mr. Glendenning has a large quantity of glass broken; so also has Mr. Stainment, and Mr. Cocks, a market gardener. The conservatories and green-houses of his Grace the Duke of Devonshire, and of the Horticultural Society at Chiswick, have suffered considerably.

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The skylights at the Euston-square railway station have suffered so much that the whole of them must be replaced.

At Walham-green (Fulham), during the storm, there was a heavy shower of small frogs, which were taken up in shovelfuls by some of the inhabitants.

At Apothecaries'-hall, about an hour before the storm burst out, a singular phenomenon was observed. In the course of the preparation of certain gases, a change in their colour was remarked. It was subsequently ascertained, from an examination of the glass, that the atmosphere was sur-charged with the electric matter, and that a storm was approaching; and it was remarked that the effects would exceed in violence any storm that had visited the metropolis for many years past.

The River (above Bridge).—During the time that the storm was at its height, the rain, which was falling in such immense bodies, ran down the various conduits into the Fleet ditch, which very speedily became filled. The outlet at Blackfriars-bridge, not being sufficiently large to allow the water to rush out as fast as it entered the sewer, the extraordinary pressure of the water blew the iron crown off the covering of the ditch with such violence that it made a noise like the explosion of a piece of artillery. At the same moment the water flew out with such impetuosity that No. 3 Waterman steamer, which was going down the river, was driven against one of the piers of Blackfriars-bridge, and so disabled that she was forced to put back and land her passengers by means of one of the Citizen steamers. The damage done to the banks of the Thames, by the rush of water, especially about Chelsea and Millbank, is very extensive. Boats were forced from their moorings, and it required the greatest exertion to keep them from being washed away.

Lambeth.—The damage done on the Surrey side of the river is far more serious than on the Middlesex side. In the well-known range of premises belonging to Messrs. Harvey, termed Lambeth-house, in the Westminster-road, the floors of the shops were covered with water. The stock in trade is most severely injured. The skylights were demolished by the hail falling upon them. The Baptist chapel in the Waterloo-road had upwards of three feet of water in it. The Hero of Waterloo inn, in the same road, had between forty and sixty tons of water in the cellar, and about sixty squares of glass demolished. The cellar-stock, of course, must have suffered. Messrs. Jones's oil-factory in Robert-street had ninety squares of glass broken. The Pear Tree tavern, in the New-cut, had the water in the cellar almost up to a man's armpits. The West of England engine was engaged nearly two hours pumping the water out. Mr. Knight, an oilman in the same thoroughfare, had a most valuable stock in the cellars spoiled by the water. The Surrey Theatre had the skylights demolished and the windows broken. The rain poured in such a manner that the place was flooded. A notice was put up that, in consequence of the damage

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done, no performance could take place that night. The surgery belonging to Mr. Brookes, in the Waterloo-road, had five feet of water in the kitchens. The goods therein were extensively damaged. The Nine-elms terminus of the Southampton Railway was extensively injured. Astley's Theatre was injured almost as much as the Surrey. Messrs. Fells' brandy distillery, in the Waterloo-road, had eighty panes of glass broken. In Bishop's-walk, Lambeth, it is stated that the lightning struck the wall of the Archbishop of Canterbury's palace, and tore up a tree in the garden. Mr. Bacon's printing premises, in Robert-street, had a number of squares of glass broken, and the business was in consequence suspended. Mr. James's grocery premises, in the New-cut, was injured to such an extent that the place was forced to be closed last night. The Victoria Theatre, and Lawrence's miscellaneous stores, at the corner of Webber-street, had some feet of water in the cellars. The neighbourhoods of Fore-street and Princes-street have suffered by the water. The inhabitants of Blackfriars-road, too, have suffered very severely, their premises being flooded and inundated to the depth of several feet. The premises of Mr. Jones, linendraper, in the Blackfriars-road, were seriously damaged, and the pavement in front of the house sunk about two feet from the effects of the rain, rendering the footway exceedingly dangerous. The roof of Davis's academy, facing the Magdalen Hospital, also sustained extensive injury, the glass being broken to the extent of nearly 40*l*. The Running Horse tavern had about six feet of water in the cellar, which completely destroyed a large stock of ale, beer, and other liquors, the rain having forced its way through one of the drains. The skylights on the roof of the Surrey chapel were considerably injured by the hail, and the pews were likewise much damaged. The houses on either side sustained more or less injury by the hail and rain, which caused a great stoppage to business. A very serious accident occurred while the storm was raging by the electric fluid striking a servant in the employ of Mr. Staff, residing in the Westminster-bridge-road. She was standing at the window, when a sudden flash of lightning shattered the window, breaking the framework and scorching her hands and arms in a most shocking manner. A surgeon was called in, who rendered prompt assistance by dressing the injured parts. She was subsequently placed in bed, and lies in a very precarious condition. Considerable damage was done to the glass on the roof of the new Roman Catholic Church in St. George's-fields. Another serious calamity took place at the residence of Mr. James Howard, fancy-box maker, residing in Herbert's-buildings, Waterloo-road, Lambeth, by which a woman named Smith, it is feared, will lose her life. It seems that Mr. Howard had a large platform fixed on the roof of his house for pigeons, and during the raging of the storm the whole of the wood-work was carried by a sudden gust of wind into a neighbour's garden at the rear of the adjoining house, falling on

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the head of the woman above named, whereby she received a severe laceration of the skull, and it is feared a concussion of the brain. She was taken as soon as possible to Guy's Hospital, where she remains in a hopeless state. Messrs. Maudslay and Field, the extensive engineers, facing the Asylum in the Westminster-bridge-road, had the whole of the glass of the roof of their manufactory entirely demolished, and the estimated damage is upwards of 200*l.* The whole of the glass on the roof of the Catholic Chapel, London-road, was broken by the hail-storm, and the interior rendered unfit for divine service yesterday. The Queen's Prison, and Horse-monger-lane Gaol were inundated for some hours, but the injury done is trifling. The glass on the roof of the museum at St. Thomas's Hospital was severely damaged, causing much injury to the interior. At Guy's Hospital the rain penetrated into one of the large wards and rose about three feet. A large body of men were immediately set to work, and were engaged the whole of Saturday night in pumping out the water. It is a singular fact that although this hospital is surrounded with glass, not a pane was broken. At Bermondsey the loss of property is very considerable. The east wing of Bermondsey Workhouse was under water for nearly twelve hours, causing great damage to the stores at the basement of the building. The violence of the rain forced the iron grating in the yard several feet into the air, flooding the place to an enormous extent. The tanners have also been great sufferers. Messrs. Cox and Drakes, of Russell-street, have suffered very severe loss, the tan-pits and warehouses being flooded; the tan-pits are entirely covered with water, in fact the premises resemble a large pond. The occupants of Finnimore's-rents, in Russell-street, were so inundated that they were compelled to make a precipitate retreat by the first-floor windows. The various licensed victuallers round Bermondsey and Rotherhithe have had their beer-cellars flooded to an alarming extent, rendering their goods quite unfit for sale, and destroying property to an immense amount. The houses in the Brixton-road present a most singular appearance, and they seem to have suffered more in this part of London than any other. Windows are entirely demolished, and the occupants in many places have placed large pieces of carpeting up to guard against the weather, until the glass is replaced, giving the houses a very singular and unusual aspect. At Bankside, all the low-lying dwellings and warehouses were placed under water the whole of Saturday night, and one poor family narrowly escaped being drowned, owing to the bursting of a sewer in Rose-court. The brickwork of the sewer ran underneath the kitchen, and while the rain was descending the floor was forced up, and the family, which consisted of an aged widow and four children, were nearly suffocated. They were fortunately rescued by several of the neighbours, but their furniture was seriously injured, and partially swept away by the flood. The vicinity of Ewer-street, Gravel-lane, was rendered quite impassable, the water in many

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parts having risen up to the first-floor windows, and the inhabitants were compelled to go to and fro by means of a large skiff. Pumps in this quarter were in active use the whole of yesterday (Sunday), and the inmates, after incessant labour, were able to reach the lower part of their dwellings, which are chiefly inhabited by the lower order of Irish. The houses in the vicinity of Blackman-street, Borough, and Great Suffolk-street, were likewise seriously injured by the rain and hail. Mr. Hinchcliff, corn-chandler, residing at No. 40, Great Suffolk-street, had a large quantity of corn, hay, and clover entirely destroyed and rendered useless by the rain, which filled his lower warehouse to an alarming extent. The house of Mr. Hotchkiss, proprietor of the Three Sugar Loaves, Union-street, Borough, was compelled to be closed, by the water entering his lower premises, entirely spoiling an immense stock of liquors which he had just stowed away for the season. Kennington Church also received very serious damage, and a large quantity of glass was broken. The church of St. Mary; Newington, sustained considerable injury, as well as the houses in the immediate vicinity.

Clerkenwell.—At the lower end of Mutton-hill the rush of the flood was so powerful that it burst through the gratings, and before the inhabitants were aware of their danger, the water, which was mixed with the most nauseous soil, rose three or four feet above the surface. The utmost alarm necessarily prevailed, but in a few minutes the cellars were filled, and those who occupied them were obliged promptly to take to flight, leaving their furniture to the mercy of the flood. At the bottom of Mutton-hill it was at a depth of four or five feet, reaching up to the house of Mr. Mombeli, carver and gilder, No. 22, Vine-street. Adjoining Mutton-hill to the north, is Wignall's place, in which are eleven houses; here the water had risen about seven feet. The utmost consternation prevailed among the inmates, who were at the first-floor windows, imploring assistance, many of the suppliants holding children in their arms. Assistance was promptly afforded by the neighbours. Among the first was Mr. Hunter, landlord of the Two Brewers, who swam to the aid of many, and rescued several children. A man named Smith, a French polisher, followed his example, and also saved many. Ladders were also brought to the spot, by means of which the women were extricated. The consequences of the flood are such that some of the houses are undermined, and cannot now be occupied. The back of the Coach and Horses, near Wignall's-place, is swept away. In Round-court, to the south of Mutton-hill, two houses, the property of Mr. Langdon, were forced down, and, but for the activity of the inhabitants, who hastened to the assistance of the inmates, many lives would have been lost. A Mr. Clarke saved three children. The premises of Mr. Godson, carcass-butcher, were nearly swept away. Among the property lost was a cart, which was swept into the sewer that is here open. In Thavies Inn the kitchens were filled with water,

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Two houses above Mutton-hill are also down. It is impossible to state with anything like precision the amount of property destroyed in this neighbourhood by the disastrous event. At Brook-hill the water continued to rise from the time the storm commenced. First the cellars were overflowed, then the water forced its way through the shop flooring, and at last, between 8 and 9 o'clock, the water was so strong that a portion of Mr. Nichol's, a furniture-dealer's premises, was washed down. The scene at that time was of a most fearful character—bedsteads, carts, tables, chests of drawers, and sundry other articles, were to be seen floating, and striking against each other on the top of the water. To save them was a work of impossibility, and the whole entered the Fleet-ditch, and were carried into the Thames. Between 12 and 1 o'clock, next morning, horses and carts were to be seen at Blackfriars-bridge, with several of the unfortunate owners, gathering up the property as it floated into the river. Several chests of drawers, a chaise cart, numerous hogsheads, a butcher's block, and sundry articles of furniture had been rescued in the presence of the reporter. The whole of the houses in Brook-hill and its vicinity were under water, some to the first floor windows. Mr. Knight, a publican in Vine-street, had his cellars so flooded that the barrels of beer were forced upwards, and destroyed the bar and parlour flooring. He also is a great loser. At 11 o'clock on Sunday night, the poor people in the courts and alleys in the neighbourhood were busily employed in removing their little furniture, as many of the houses, in consequence of the water acting on and sapping the foundations, which were very old, have sunk several feet, and are rendered extremely dangerous. A strong body of police afforded most efficient assistance in this distressing affair. The authorities of Clerkenwell workhouse gave a general order for the admission of the poor people and their families who have been rendered homeless. Up to 12 o'clock it was not ascertained that any lives had been lost, although three children were supposed to have been drowned. After every practicable search, the prevailing opinion is that such is, fortunately, not the case.

Windsor, Sunday Evening.—The storm of yesterday, which appears to have been general throughout the country, commenced here about half-past 2 o'clock, and continued up to past 5. The hailstones had done considerable damage in the neighbourhood, especially in the vicinity of Datchet and Horton. At Ditton-park, the residence of the Duke of Buccleuch, upwards of two thousand panes have been broken; nearly the whole of the extensive hot and green houses have been totally demolished. At Slough also, and the neighbourhood of Burnham, the hail which accompanied the thunder and lightning, has done very considerable damage. A great portion of the standing corn has been beaten down, and much injury has been sustained by the market-gardeners in every direction. At one o'clock, yesterday, shortly

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before the storm commenced, the thermometer, in the shade, in the Royal gardens, at Frogmore, stood at 85°.

Camden Town.—During the violence of the storm the electric fluid struck the house of Mr. Corvan, No. 17, Mornington-road, Camden-town. This took place about 4 o'clock, the shock causing the utmost alarm to the neighbourhood. The fluid entered the chimney, which it shattered to pieces, and then passed down the chimney flue into the kitchen, in which was the female servant, who was at the time occupied in cleaning some plate. She became senseless, and was immediately attended to by Mrs. Corvan, who had her removed, as it was not possible, in consequence of the strong smell of sulphur, to remain there. In the mean while numbers of the inhabitants hastened to the house, as it was reported that it had caught fire, and there was a strong smell of wood burning. Upon an examination of the roof, the timber under the slating was found extensively charred; but as it was raining in torrents at the time, the progress of the fire was soon extinguished. A quantity of lead was also melted. The houses on each side were also slightly damaged. In the Regent's-park, above the Zoological-gardens, a portion of the bank of the Regent's canal was washed away, some trees being forced down. In the New-road, between King's-cross and Judd-street, in consequence of the sewer being overflooded, it gave way, causing a considerable extent of damage. The thoroughfare was, of course, instantly stopped.

In *Bromley* and the immediate neighbourhood very much damage has been done to the several nurseries by the hail, several thousands of panes of glass being demolished. One of the hailstones or rather pieces of ice which fell at the latter place, when measured, was found to be of an angular shape, and upwards of three inches in circumference, the greatest width at the top being one inch and a half. So far as can be ascertained, no lives have been lost in the range of locality previously described.

Greenwich, Deptford, Lewisham, &c.—The coming storm was indicated by very heavy and dark clouds from the south-west, about 3 o'clock, followed in a few minutes by vivid lightning and claps of thunder, resembling a park of artillery in the distance. Then came torrents of rain, succeeded by a fall of hailstones, which continued with a few intervals of short duration until 6 o'clock. As the storm advanced over these places, the lightning increased to an awful extent, and between the flashes the clouds were so heavily charged that there was the appearance of approaching night. About an hour after the storm commenced, the lower parts of Greenwich and Deptford, were inundated. The flood which poured from Shooter's-hill and Blackheath, down Croom's-hill and Royal-hill, completely choked up the drainage, and caused London and Church-streets to be completely under water. The kitchens and cellars in this line down to the Royal Hospital, and alongshore to the creek which divides Greenwich from Deptford, were in a

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most deplorable state from the same cause. The lower part of Blackheath-road, Deptford-bridge, and Broadway, were impassable for foot-passengers during two hours. The vicinity of the dock and victualling-yards, the General Steam Navigation yards, Penn's boiler manufactory, and other extensive works, were surrounded by water at least a foot deep. The devastation caused by the extensive and heavy fall of hail is very great. Numerous skylights and windows were demolished in the dock and victualling yards. At the Greenwich police-court, the police-station, and premises attached, much glass was broken, and damage done by the water getting into the apartments. The skylights and windows in the factory of Messrs. Penn and Son, engineers, Blackheath-road, suffered severely, upwards of two hundred squares being smashed in, together with portions of the frame-work. More than one hundred squares of glass were destroyed at Messrs. Weelhouse's distillery, Deptford-bridge; and nearly the whole of the glass frames and greenhouses in the market garden and nursery of Mr. Loft, near the Kent Waterworks. Much damage was done also to the glass work and choice productions of Mr. Cormack's nursery, at New Cross. At the police-station, Rushey-green, Lewisham, much glass was broken, together with the frame-work thereof; and the lower part of the village between Lewisham-bridge and Lea-bridge was so effectually under water that it was with some difficulty horses could ford it, particularly if attached to carriages. The market gardens at Lewisham, Lee, Deptford, and Woolwich-road, have suffered immensely, not only from the hail, which at Lewisham fell in flakes an inch long, but from the flooding of the growing crops. Throughout the whole district of Blackheath the hailstones fully averaged the size of large hazel nuts. The gardeners who have suffered most are Mr. Tyler, near the Greenwich Union; Mr. Shepherd, Lewisham; Mr. Palmer, Lewisham; and Mr. Loft, Greenwich. The quantities of sand and gravel which have been washed from the rubbish-hills into Greenwich and vicinity is astonishing, leaving the high roads bare, and presenting nothing but a surface of bright and broken granite. On the Eltham-road, a man with a scythe in his hand was struck by the electric fluid, and fell insensibly to the ground. The poor fellow, however, luckily recovered so as to be able to proceed home.

Plumstead.—In the midst of the storm the electric fluid struck one of the chimneys belonging to a house in the occupation of Mr. George Kimber, stone-mason and marble-cutter, next door to the Plume of Feathers, opposite to the church, at the village of Plumstead. From a conversation held yesterday morning with Mr. Kimber's son, by the reporter, it appears that the moment he, with his father, was engaged in preparing for polishing a marble tablet, and that, whilst thus engaged, a stream of fire passed through the shop, which is on the first floor on the back of the house, entered at the ceiling, which it tore down with great violence, as it also did the plaster of the side walls. Thence it passed through the floor to the

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cupboards below, the woodwork of which was demolished, and the crockery and miscellaneous contents therein destroyed. It then appeared to pass through a chink in the wall to a butt in the yard, which was broken up, and after playing in sportive fury for some time in the roadway, it appeared to pass into the ground, rising out again at a distance from the house. Full half the chimney was torn down, and the bricks fell into the yard among several gravestones, in a course of preparation. Two large holes were made in the tiles, which penetrated the roof, from the rafters through the ceiling. Mr. Bingham, the landlord of the Plume of Feathers, states that he was standing in front of his bar, serving customers, at the time when the flash of lightning—or, rather, the stream of fire, appeared. So fierce was the flame, and so appalling the report arising from it, that he was for a few minutes unconscious where he was, or what he did. The next moment he heard the crash of the falling brickwork, and, upon looking out of the window, after his stupor was in some measure subsided, he saw a volume of dust proceeding from the roof of the premises in Mr. Kimber's occupation.

At *Erith*, *Crayford*, and *Dartford*, the storm was very severe; and at Chatham it is said to have been excessively violent. In the midst of this visitation and loss of property, it is gratifying to state that the hop bine has much improved since the storm, one of the largest growers in the west of the country having discovered, upon an examination of his crops that, although on Friday last they were covered with fly and lice, the thunder and lightning had cleared the majority of the arms from these destructive vermin.

Southend.—The damage that has been sustained by the hail-storm which fell on Saturday is tremendous. The direction the storm took being from south to north, the hot-houses and green-houses presented a fair mark for its fury, and the destruction that followed it is most extensive. Mr. Samuel Forster, of Southend, has his beautiful hot-houses completely riddled, there not having been left more than one-tenth of the panes whole. The number of broken squares amounts, in this place, it is said, to over 4,000. Mr. Hill, of the same place, has had the front of his house completely disfigured, nearly all the beautiful plate-glass windows having been broken.

The violence of the storm extended itself to the neighbouring counties:—in the neighbourhood of Reading, Oxford, Newbury, Farringdon, Cirencester, Wantage, &c., the storm was of a most awful description, and the damage sustained to vegetation generally is incalculable. At Cirencester two sheep and an ox were killed by the electric fluid; and at Wantage two sheep and a valuable horse were struck dead by the same powerful agency. At Oxford the electric fluid did great damage to building property, hot-houses, and gardens. The roof of the house of Mr. Dudley, situate near the river, was struck by the lightning. At Letcomb, near Wantage, the lightning entered the roof of a cottage, shattered a bedstead,

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and threw a child that the mother had just laid in bed, on the floor, but unhurt. At Highworth, the servant of a Mr. Boyne got under a tree, with his horse, for shelter; the horse was killed, but the man miraculously escaped with a slight scorching of the forehead. At Henley-on-Thames the flashes were awfully vivid and forked. The house of Mr. Musgrove was struck by lightning. Mr. Musgrove and a portion of his family were at the moment standing in a passage through which the bell-wires passed, and were all struck to the ground, but happily without serious injury; the damage to his out-houses, plants, &c., contiguous to his dwelling-house, is very considerable. Some parts of Hampshire, &c., were also visited by the above storm, and serious damage was sustained by the electric fluid. At the Swan Inn, Inkpen, a small village about three miles from Hungerford, the paper on the walls was in many places completely stripped off. The lightning then followed the bell-wire to the kitchen, where the maid servant was at work; her clothes caught fire, but her screams speedily brought assistance, and the flames were happily extinguished without her receiving much injury; and a boy, who was standing in a barn near the house, was knocked down by the lightning, but escaped with a few slight scars on the cheek; a great portion of the barn, however, was destroyed by the electric fluid.

The Storm in Birmingham.—Birmingham, Saturday.—This afternoon Birmingham and its neighbourhood were visited by a dreadful thunder-storm which lasted upwards of two hours. The wind blew gently from the south-west, from which direction the tempest came. The rain descended in torrents, and in some parts of the neighbourhood the flood rendered the roads impassable. Fortunately, although at one period the storm was of the most fearful description, no serious damage was done in the neighbourhood.

LETTER TO THE EDITOR OF THE TIMES.

SIR,—As many of your readers may feel an interest in circumstances connected with the hail-storm on Saturday, I beg a corner of *The Times* for a few observations that may amuse them.

The small garden of the house in Camberwell New-road, in which I was when the storm commenced, has an easterly aspect. There was no wind at the moment, and there was no perceptible drift in the shower; the hail seemed to fall in a direction nearly vertical; and from the nature of the surface of the garden, it rested where it fell. From a spot measuring three superficial feet, and which was selected as a fair average of the general surface of the garden, I gathered 947 hailstones, the smallest of which weighed 9 2-10ths grains, and the largest 84 grains.

I was anxious to ascertain the number of the different sized hailstones which fell on a given space, as I observed they varied greatly in their dimensions; more so on this occasion than in any other hail-storm that I ever witnessed.

They were of five sizes: there were 61 of the largest size. Five

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of them, hastily selected, as far as the eye could judge, nearly of the same size, weighed 405 grains, which gives 81 grains as the average weight of each. Five others, apparently the smallest of the number, weighed 394 grains, which averages 78 grains each. One of the first five measured 9-10ths of an inch in length, 6-10ths of an inch in breadth, and was about 5-10ths of an inch in thickness. It had, like most of the others, the shape of a flattened pigeon's egg. A dozen had a shape approaching a flattened sphere. In general, it may be observed, that no two hailstones, although nearly of the same size, had the same form, or seemed to have been produced under the same circumstances.

417 hailstones were of the second size. 10 of these weighed 519 grains, which gives 51 9-10ths grains for a single one. The 10 smallest that could be selected from the whole number had an average weight of 50 2-10ths grains each. The uniformity of size and shape, throughout this large number, was very remarkable; they were mostly like flattened spheres, with a thickness of half their diameter. They could hardly be distinguished from small white turnip radishes trimmed of their root fibres and leaves.

285 hailstones were of a much less size than the preceding. 17 of them weighed 651 grains, which gives 38 2-10ths grains to each. The smallest that could be selected from the whole number, by the eye, weighed 31 grains. Their forms were all imperfect; some were roundish lumps, others like short contorted cylinders, others had an almond form.

Of the fourth size there were 159 hailstones; 36 of these weighed 828 grains, or 23 grains a piece. The smallest of the whole number weighed 18 grains.

The largest of the 16 hailstones of the fifth size weighed 13 grains, and the smallest 9 2-10ths grains. The shapes were most irregular; they had much the appearance of being chipped from larger pieces, with their sharp edges rounded by the warm rain in which they descended.

9 hailstones of the largest size had a squarish form. One of them was nearly an inch long, 7-10ths of an inch broad, and was fully 5-10ths of an inch thick. Two of its edges were ragged, and looked as if it had been part of an irregular cake, composed of several thin plates of soft ice pressed together, and then broken off; the other two edges had a similar appearance, but their arrises were somewhat rounded. It weighed 87 grains. Of a similar shape and appearance were four others, the average weight of each of which was 76 grains.

Of the remaining four hailstones it is difficult to give an idea. They had the appearance of a portion of snow that had been firmly pressed and flattened in the hand, and then dipped in water, but they nevertheless seemed as dense as the hailstones of a more regular shape.

Many of the largest sized hailstones had a portion, as much as a third or a fourth, broken off. This in some instances might have

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been produced after they had fallen, by the impact of those falling, but not in other instances. I observed the same thing in some large hailstones which fell into a tub of water, and where an impact with others could not have taken place. The disruption of these must therefore have been produced in their descent through the atmosphere.

In general the largest hailstones seemed to have the greatest density; several of those which I have described as of the fifth size were spongy like pressed snow.

I have no doubt but that the fall of such large hail will have occasioned much damage to gardens and windows, but I scarcely remember a hail-shower where the momentum of the hail was felt to be so small. Perhaps the falling torrents of rain may have, in many cases, diminished the impetus, by deflecting the hail, and by occasioning an upward current of wind from the ground, like that produced by the fall of water in the chamber of a wind furnace, and which neutralized, in some degree, the effect of the descending hailstones. I am, Sir, your obedient servant,

August 2nd. M.

CLXVII. *Greener and Staite's Electric Lamp.* From Mechanic's Magazine.

We have here another attempt to realize that favourite speculation of the day—a lamp which shall burn without air or oxygen, and be fed solely by that mysterious agent which we call Electricity. Not long ago we gave a place for the purpose, of American origin, patented in England by a Mr. King, and felt obliged to speak of it in terms which have not been gainsaid by anything we have since heard. The present has a more promising aspect. It shows at the least, decided originality. We shall give the Patentee's description in their own words.

"The nature of our invention and the manner in which the same is performed, may be stated generally as consisting in effecting the illumination of public and private buildings, streets, squares, and other public places, by means of solid prisms or cylinders of carbon, enclosed in air-tight vessels of glass or some other transparent substance, and ignited or rendered luminous by currents of voltaic or magnetic electricity, such carbon being previously freed from the impurities with which it is ordinarily combined, and divided on the surface thereof into numerous acute points—or by means of rods or strips of platinum or some other difficultly fusible metal enclosed in transparent air-tight vessels, and ignited or

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rendered luminous by currents of voltaic or magnetic electricity as aforesaid, such metal being also divided on the surface thereof into numerous acute points—or by means of hollow cylinders of carbon with surfaces either plain or acuminated, partially inserted within, and placed in perfect contact with hollow cones of platinum, either plain or acuminated, the whole being enclosed in transparent air-tight vessels as aforesaid, and the carbon being rendered luminous by voltaic or magnetic electricity, also as aforesaid—or by a combination or combinations of the whole of the said means or methods—or of any two thereof, or of any of the parts thereof respectively. Both carbon and platinum have been before employed as media for the development and exhibition of electric light, but carbon, even in the purest states in which it has been hitherto obtainable when ignited, or rendered luminous in an air-tight glass vessel by means of electric currents, has been found to give out various extraneous matters which interfere with the continuity of the light, and, being precipitated on the inside of the glass vessel, obscure and darken the same; and plain surfaced platinum, when substituted for the carbon in the air-tight vessel, has never yielded more than a comparatively very feeble light.

Now to obtain for the purpose of our invention a carbon absolutely pure, or at least more nearly approaching absolute purity than any heretofore known, we proceed as follows:—

We take a quantity of what is known in commerce under the denomination of lamp black, or of charcoal reduced to a state of powder, or of coke, also reduced to a state of powder, which has been purified by the application of electricity from sulphur and other metallic mixtures, according to the process for which Jabez Church, of Colchester, has obtained letters patent, of date the 20th December, 1845, and digest the same in dilute nitro-muriatic or other acid. We then strain the lamp black, or charcoal, or coke, from the acid, and wash it several times with water; we next wash it in some weak alkaline solution or carbonated alkaline solution, and, finally, we wash it with distilled water until no traces of impurity are perceptible. The carbon thus highly purified, we next bring into a state of great dryness, and then convert it into solid prisms, or into cylinders, both solid and hollow, by means of a hydraulic or screw fly press (in the same way as the well-known porcelain buttons are formed). And sometimes the better to secure perfect dryness and homogeneousness, we expose the said prisms, or cylinders, to intense heat in a furnace for about twenty-four hours.

To give the solid carbon prisms or cylinders the numerous

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acute points on their surfaces before mentioned, they are cast into suitable indented discs, or the points are afterwards cut out in them by means of a fine saw. By opposing two such acuminated surfaces, the one to the other, we secure this advantage, that should the electric current cease between any two points (as happens frequently in the ordinary mode of igniting carbon by electricity, when there are only two points in opposition) the currents will be kept up by the remaining points, and the light be so maintained, without any material interruption or abatement. The same purpose may, in some cases, be effected by substituting for bodies with such acuminated surfaces a multitude of thin strips of charcoal separated from each other by platinum foil and bound together by platinum wire. Instead also of employing either lamp black, or charcoal, or coke, we sometimes substitute plumbago, or black lead, prepared, formed, and used in the same way; but the sort of carbon which we prefer before all others, is that prepared in the manner before mentioned, from coke purified by Church's patent process.

When we use platinum, or any other difficultly fusible metal, as the illuminating medium, we multiply as much as we conveniently can, the number of separate points on its surface; and with this view we either perforate a solid sheet of the metal with a multitude of minute holes, or we take it in the form of wire, and weave it into a gauze of sufficient thickness to resist the heat produced by the electric current without fusing. When we employ the combination before mentioned, of hollow cylinders of carbon, with hollow cones of platinum we arrange them as follows:—

Supposing a voltaic battery of any of the most approved forms to be employed, we connect each of the two conducting wires, namely, the positive and negative, with a hollow tube of platinum, either plain or acuminated, and place the cones with their bases facing, but at a little distance from one another; we then connect the two cones by placing between them a hollow cylinder of carbon, either plain or acuminated, and insert it so far into the cones at the two ends, as to be in perfect contact therewith all round. The relative diameters should be such as to render it necessary to introduce the cylinder but a small way into the cones. Such cones or cylinders may be used either in single sets, or in any number of sets which the battery or batteries employed may be adequate to supply. The sets, too, may be arranged in any order, and they may be of any variety or varieties of size best calculated to produce an equable, well sustained, and brilliant light. When the carbon, or platinum, or any other difficultly

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fusible metal (in whatever form employed) is placed within the air-tight vessel of glass, or other tansparent substance, it may be either kept stationary, or made to rotate on its axis.

We think it unnecessary to describe either how the air-tight vessel which is to enclose the illuminating bodies should be mounted, or how the electric currents are to be conveyed to them from the voltaic battery, or magnetic machine employed, because there are many well-known modes of doing so, and we do not claim to have invented anything new in these respects. We desire it further to be understood, that we lay no claim to the employment of carbon, or platinum, or other like difficultly fusible metals, enclosed within air-tight vessels, as a medium for the development and exhibition of electric light, the suitableness of both bodies for the purpose, when so enclosed (subject to the exceptions aforesaid) being long well known. But we declare that what we do claim as of our invention is, *first*,—the employment as a medium for the development and exhibition of electric light, of carbon purified and prepared as before described. *Second*,—the employment, for the said purpose, of carbon and platinum, or any other like difficultly fusible metal in solid prisms, or cylinders, divided on the surface thereof into numerous acute points, as before described: and, *third*, the employment for the said purpose of hollow cylinders of carbon, either plain or acuminated, in combination with hollow cones of platinum, or other like difficultly fusible metal, either plain or acuminated, as before said.

CLXVIII. *On the Action of Magnetism on all Bodies.* M. Ed. Becquerel. Comptes Rendus, June 8th, 1846. Vol. xxii. p. 952. No. 23.*

(TRANSLATION.)

A few months ago Mr. Faraday discovered that a powerful electro-magnet may act upon a transparent substance so that, if a polarized ray traverses this substance in the direction of the line of the poles or the magnetic axis, the plane of polarization is deviated either to the right or the left, according to the direction of the magnetization. M. Pouillet was the first to repeat these experiments in Paris.* Since this period a great many philosophers have hastened to follow his

* Vide *Elec. Mag.* vol. ii.

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example; but hitherto, as far as I know, the action has not been sufficiently powerful to permit of our completely analysing this phenomenon. I have, therefore, greatly endeavoured to increase these effects, and I think I have succeeded in this by employing an enormous electro-magnet, and in arranging the substances in a manner different from that hitherto adopted.

Before indicating the results obtained, I will describe rapidly the apparatus that I employed. The electro-magnet is formed of a cylindrical bar of soft iron one metre in length and 11 or 12 c.m. in diameter, bent as a horse-shoe; the two branches are distant from the interior 14 c.m., and the iron bar weighs 63 k.g. Around its two branches, and parallel with them, are coiled two copper wires covered with cotton, 910 metres in length, and 2 m.m. in diameter, so that the current can pass either in a single wire of 1820 metres in length, and 2 m.m. in diameter, or else in two wires of 910 metres. The weight of the copper of these wires is 50 k.g. The electro-magnet is solidly fixed on a tube, so that the flame passing through the two extremities of the soft wire is horizontal and level with the opening of the screen of a dark chamber. On each branch of this horse-shoe, a parallelopiped mass of soft iron is placed, of the same width as the diameter of the iron, namely 11 or 12 c.m., and 16 c.m. long, and 5 c.m. high. These masses of iron are pierced in the central part, along their whole length, by a cylindrical opening of 2 c.m. in diameter; and they were arranged so that the two openings and that of the dark chamber are the prolongation of each other. The masses of soft iron may be approached or separated from each other without the openings ceasing to correspond, and this motion permits of approximating, to a greater or less degree, the contrary poles, which by induction are developed upon their opposite faces; we may thus vary their distance from zero to 14 c.m. The substances upon which we desire to operate are placed between the pieces of iron, so that the poles of these latter acting normally, the magnetic axis is found to be the direction of the luminous ray, as the phenomena are observed through the longitudinal opening of the pieces of soft iron. These masses, magnetized by induction, greatly increase the effects, and as their polar intensity increases, in proportion as they are brought nearer together, the action of magnetism may be rendered evident upon transparent plates of a few millimetres in thickness. We also employed armatures arranged in order to increase the effects of the magnetism; these armatures, which must be

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pierced along their whole length, as the former are, give in certain cases very energetic effects.

The electro-magnet being described, we operate in the following manner: the white light of the clouds was introduced into the interior of the dark chamber by a reflector placed at the opening of the screen, at a certain distance; the rays are received upon a Nichol's prism, situated in front of the electro-magnet, and which may be considered a polarizer. I also employed light, polarized directly by aid of a glass inclined at the angle of polarization, and the effects are precisely the same. The polarized light traverses the two masses of iron, and the transparent substance placed between them; it is then received on the other side of the electro-magnet, on a second Nichol's prism, or on a bi-refracting prism adapted to the centre of a circle, divided perpendicularly to the direction of the luminous rays. A Nichol's prism is employed when the effects to be observed are feeble; and the bi-refracting prism in the contrary case. Rotating movements permit of turning the prisms in all possible azimuths. I call *objective prism* the one which is placed in front of the electro-magnet, and which gives the direction of the plane of polarization; and *ocular prism* that which is placed on the side of the eye.

1st.—*Effects produced on non-crystalline substances.*—In order to observe solids, they are cut into the form of a screen with a parallel face. With regard to liquids, they are placed in small water tubes, or in screens of very thin glass. Suppose that the electro-magnet is excited by a carbon and nitric acid pile of forty elements strongly charged, we then see that all transparent substances placed between the masses of iron are influenced by the magnetism. If the ocular prism is turned so as to extinguish the image of the opening of the screen before magnetization, immediately after the passage of the current, the transparent substance is influenced, and the image appears again with more or less intensity, according to the nature of the substance; if the current cease, the image disappears. A person placed in an apartment next to the dark room may, by means of a commutator, pass the current into the electro-magnet, intercept it, and change its direction. To prevent the two masses of iron, on account of their magnetization, from pressing the substance placed between them, we place in the interval a piece of wood of a little greater thickness than that of the body, and pierced in the centre, like these masses of iron, so that the wood alone receives the compression. When the image of the opening of the screen of the dark chamber, and which was extinguished by the

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crossing of the Nichol's prism, appears in consequence of the action of magnetism upon the substance, it is sufficient to turn the ocular prism from the right to the left, or from left to right, according to the direction of the magnetization, in order to extinguish it anew; but this complete extinction can only be obtained when the effect is very feeble. Mr. Faraday announced that the phenomenon was a rotation of the flame of polarization; he found that the direction of this rotation did not depend on the substance, but on the direction of the magnetic axis; and that when the austral pole was on the side of the observer, it occurred toward the right, whilst it occurred toward the left when it was the boreal pole.

When we operate with white light, as I did, at the moment when the magnetic poles influence the substance, the image of the opening of the dark chamber appears coloured of a bluish-white. If the ocular prism is turned, so as to diminish the intensity of this image, it appears blue before zero, and red after, whatever be the direction of the rotation. I have operated by aid of prisms of heavy glass (silico-borate of lead) prepared by Mr. Faraday, and which MM. Biot and Dumas were kind enough to confide to me. Heavy glass is the substance most influenced by magnetism. These prisms, one of which is 48, and the other 46 m.m. in length, each give between the masses of magnetized iron about sixteen degrees of rotation. With a stronger pile, much more energetic effects are obtained; but I had not one at my disposal. These prisms, like all other melted substances, are annealed; it is then necessary, as Mr. Faraday remarks, to act upon their central part, which presents the black cross, when the polarized light has not experienced modification. To attain this end, the opening of the screen of the dark chamber is reduced, and the ocular prism is brought near to the heavy glass; the crossing of the Nichol's prisms will then extinguish the image of the opening of the screen. When the current passes, this image immediately appears coloured: on turning the eyepiece, a succession of divers colours was observed, from clear blue to pale yellow; and the impression felt is the same as if we had placed in the course of the polarized ray a plate of quartz two-thirds m.m. in thickness; or, rather, a tube of water slightly sugared. On replacing the Nichol's prism eyepiece by a bi-refracting prism, and then turning this prism on the divided circle, the images were seen successively to change their tint in the four quarters of the circumference. The colours indicated an unequal action on the part of the medium influenced upon the different luminous rays composing white light; it was important, therefore, to

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know whether the law of these inequalities is the same as that which was discovered by M. Biot in the substances naturally endowed with rotation. M. Biot, in fact, found that in quartz the rotation is to the right or to the left, which, according to the specimens, increases proportionately to the thickness, and varies also for the different simple rays; the angle of rotation, in the greater part of active substances, goes on increasing with the refrangibility, according to a proportion sensibly *reciprocal to the square of the length of the undulations proper to each species of ray.* When we seek for the rotatory power of several bodies, as in general their dispersive power is not similar, the relations between the angle of rotation of the different simple rays are not the same; but if we operate by means of two substances of the same dispersive powers, the order of the colour must be the same. It follows from this that, if the two substances have equal and contrary rotations, they may be able to destroy each other completely.

When the rotation of a substance is observed, and the ocular prism is turned, we arrived at an indigo violet tint, which M. Biot termed *transition tint.* This tint, for the least movement of the ocular prism to the right or to the left, passes to red or to blue, which renders it easily perceptible. When it has been obtained, and there is no deception to half a degree of its value, we may be sure that the angle through which the prism has been traversed from its primitive position corresponds to the rotation of the complimentary tint, or the mean yellow, the most luminous part of the operation. After having determined the rotation of the heavy glass in consequence of the action of magnetism, that is to say 16°, we prepared, after the manner indicated by M. Biot, a tube of sugared water, that had the same rotation on this glass. If this tube is placed between the eye-piece and the electro-magnet, and the current is made to pass successively in both directions, the glass will be influenced; we shall observe no effect when the rotations are inverse, but we shall have a double rotation when they act in the same direction. In the former case, we shall no longer see colours; in the latter, the rotation will be 32°. These results show that the effect produced by the action of the magnetism is a rotation of the plane of polarization, and that, for the different simple rays, the law is sensibly the same as that which has been given by M. Biot for quartz, sugar, &c.

I have made an experiment, the result of which came out evidently from the experiments made by Faraday in the helixes, and those of M. Pouillet. I placed the Nichol's prism object-glass, so that the plane of polarziation was suc-

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cessively in all azimuths, and I always found the rotation constant, whatever were the direction of the primitive plane of polarization.

When I operated by means of screws of the same substance, and of different lengths, I was surprised to see that a prism of 1 or 2 c.m., placed between the pieces of soft iron, which were brought near so as almost to be in contact with it, gave an action as strong as a prism of 10 c.m. placed in analogous circumstances. I repeat,—this occurs because, in proportion as we approximate the masses of iron, the polar intensity increases to such an extent that the increase of magnetic intensity compensates for the diminution in the length of the substance acted upon; but, if we leave the two pieces of iron at the same distance, and place between them prisms of divers lengths, the effect increases with the length. There is no advantage, therefore, in using a thickness greater than a few centimetres. Guided by these considerations, I formed species of piles by alternating pieces of soft iron pierced, and screens of the active substance 1 c.m. thick; but, although I obtained more marked effects, I did not find any great advantage in operating thus. Perhaps, by having a more powerful voltaic pile, or rather by surrounding the systems, soft iron and active substances, by the wire of helixes, we might obtain much more satisfactory results; moreover, *à priori*, we conceive that, in the alternations of little tubes of iron and screens, the effects must be more marked than in one continuous hollow iron tube. The different species of glass are not equally impressionable; Faraday's heavy glass is the substance most powerfully influenced, but then comes flint, and finally crown; but almost all the specimens that I employed are annealed; it is, therefore, necessary to take the precautions indicated above.

All the liquids that I have been able to try have presented me with an action; but it is very different according to the substance, as Mr. Faraday announced in his memoir. The chlorides show the phenomena and also the beautiful colours of which I have spoken in reference to heavy glass very distinctly. In one experiment, I had for the rotations very pure liquids, placed in a small screen 1 c.m. in thickness; chloride of zinc dissolved, 6°; chloride of calcium and sodium, both $4\frac{1}{2}$° nearly; pure water, 3°.

These numbers must not be considered as measures, but as the indications which show that the effect changes with the nature of the liquid.

2nd. *Crystallized Bodies.*—Mr. Faraday, from his experiments, says that crystallized bodies seem to refuse the rota-

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tory action of luminous rays under the influence of magnetism, and the pieces of quartz, carbonate of lime, &c., perpendicular to the axis do not present any effect. I have thought that this might perhaps arise from the primitive action of these substances upon the polarized rays being such that it is difficult to appreciate differences of 1 or 2 degrees in the rotation. The two following examples support this manner of viewing the subject. To annihilate the primitive effect of the crystals of quartz, I chose two plates of contrary rotation and of equal thickness, which completely neutralize each other, by placing themselves perpendicularly to the polarized ray. Immediately the electro-magnet began to act, the effect was manifested as upon a plate of glass, sometimes in one direction, sometimes in the other, according to the direction of the magnetization; but I must confess that it was much more feeble than in the latter substance. Each plate was nearly 5 m.m. in thickness. I also operated by striking together two plates analogous to the preceding, and which were such that they gave the transition tint for the crossing of two Nichol's prisms (this was one of M. Soleil's double rotating systems, being about 6½ m.m. in thickness). When the magnetization has occurred, the tints of the two plates are seen to change; one turned to blue, the other to red; thus, even under these circumstances, the effect produced on the rock-crystal was appreciable.

I submitted to experiment bi-refracting crystals that are not endowed with rotation; I first operated with the beryl. M. Biot kindly entrusted to me several species of this substance. One of them, yellow, 1 c.m. in thickness, placed perpendicularly to the axis, showed the black cross in the centre in a very distinct manner. Between the magnets, the action on the black cross appeared sensible, although feeble, and yet, on measuring it, the rotation was found to be more than 5°. I repeat,—this effect occurs from the energetic action primitively exercised by these crystals upon polarized light, in consequence of their molecular constitution, and which makes a very feeble rotation on a slight change in the position of the ocular prism almost inappreciable. The other specimens of beryl, carbonate of lime, and tourmaline could not be observed for these reasons. Magnetism, therefore, influences bi-refracting crystals, as well as amorphous substances; only the effects are more difficult of observation.

3rd. *Reflections on the action of Magnetism upon all Bodies.*—The results obtained by means of the electro-magnet described above confirm, therefore, the opinion that the phenomenon discovered by Faraday, is a rotation of the plane of polarization impressed upon a body by the influence of mag-

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netism, and the direction of which depends upon the direction of the axis of magnetization, and which must always be that of the luminous ray, by means of which the effects produced are analysed. These results also show that the laws of the rotation of the planes of polarization of the different luminous rays are sensibly the same as those which have been given by M. Biot for substance whose molecules are endowed naturally with the rotatory power; but there is this difference between these latter and those which are influenced by magnetism. That, in the former case the phenomenon is molecular and does not change with the direction of the luminous ray through the substance, whilst, in the latter case, the rotation depends upon the direction of the magnetization.

I, therefore, think, and it is the most reasonable consequence resulting from my experiment, that these effects are due to a molecular change in the state of bodies—a change which must be symmetrical all around the direction of the line of magnetization.

In the course of these researches, I was struck with a phenomenon that I do not think I can explain in the same manner as Mr. Faraday. When the current is made to pass in the wire of the electro-magnet, the image produced by the rotation of the plane of polarization, does not immediately acquire all its vivacity, but gradually increases in intensity. When the current is destroyed, the image suddenly disappears. Mr. Faraday thinks that the gradual increase of the image is due to the time the interior iron cylinder requires in order to attain its maximum of magnetization, and that the more intense the magnetism becomes the more the light increases. But, if the magnetization does not immediately acquire its maximum, no more can it suddenly lose it, and the time necessary to return to zero when the current is interrupted must be at least the same as that which passes between the moment when the current is established, and the instant of the maximum; thus we ought to see the image gradually return to zero, in the same manner as it became gradually visible. As this is not the case, I should be induced to believe that the effect is due to the action exercised by magnetism on the molecules of the bodies, and arises from the latter requiring a certain time to attain this new state of equilibrium; when the force ceases to act, the equilibrium is destroyed. What occurs under these circumstances is analogous to what is observed when a spring is stretched; it unstretches faster than it stretches.

I cannot come to a termination of this note without saying a few words on certain experiments made by Mr. Faraday,

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touching the action of magnetism upon all bodies, and which have no relation to the fact of the change in the optical properties of these bodies, but rather to the direction of small needles of any substance placed between the poles of two powerful magnets.

Coulomb was the first to observe that all bodies obeyed magnets, but more feebly than iron, nickel, and cobalt. My father, in 1827, took up the question at the point where this celebrated philosopher had left it, and observed, moreover, under certain circumstances, instead of placing themselves in the line of the direction of the poles, these new small magnetic bars placed themselves perpendicularly or obliquely to this direction; finally, according to their position and their distance from the poles, they may be placed in all possible directions. These experiments were made about twenty years ago, and yet Faraday, assuming anew the phenomena of transverse direction, has made of the bodies which thus place themselves a new class of substances, and has named them *diamagnetic substances*. Moreover, M. de Haldat, in a small work on magnetism, after having spoken of Faraday's researches, and his own, without speaking of the preceding, which are very much anterior to them, adds that he should not be far from admitting a third class of substances, directing themselves obliquely, and which seem to form a mean term between the two others, on account of the small appreciable quantity of iron that they contain. I ask how, under these circumstances, the spirit of classification can be thrust so far, when to the same substance can be given all the diverse positions—longitudinal, transverse, and oblique. In fact, when we place opposite to each other, at two or three m.m. distance, the two extremities of two strong magnets, and suspend at about one m.m. from their surface, by means of a thread from the cocoon, a small wooden or copper needle, &c., of one m.m. in diameter, and five or six c.m. in length, it places itself transversely. If this small needle is cut into two, and then again into two, &c., we at last finish by having a fragment that places itself in a direction of the line of the poles. This is very simply a phenomenon of the resultant; for to the same substance may be given those divers positions, according to its form, by modifying the distance of the poles.

We do not yet know what relation exists between the change of the optical properties of transparent substances, and the intensity of magnetic action given to these substances by the action of magnetism; but it is probable that these two phenomena are connected with each other. If it were so, and then that iron, metal, and cobalt had been

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transparent, we should have found them endowed with properties relatively much more energetic than the other substances.

Coulomb, after having found that all bodies obeyed the action of magnetism, attributed this phenomenon to the presence of iron, and inquired not what quantity of this metal was necessary to give rise to the effect observed. He found that the presence of $\frac{1}{133120}$ of iron was sufficient to explain them. About a year ago, in a memoir that I had the honour of presenting to the Academy on the same subject, (*Comptes Rendus*, June 9, 1845; the memoir has not yet been published) I found a much smaller number than Coulomb, and I arrived at the conclusion that the action of magnets upon transparent substances to attract and direct them change with the purity of the specimens, and that in certain cases (silicum, and iodine, &c.) we succeed in obtaining small needles which are scarcely influenced by the magnets. (It is possible that the same may be the case in respect to the optical modification discovered by Mr. Faraday, and that divers specimens of the same substance, quartz for example, present different actions.) I then said, at the end of this memoir, that these substances act as do mixtures of hard substances, and magnetic particles; and that it is probable that, in a great number of cases, the effects observed are due to mixtures of iron or of ferruginous compounds. These conclusions remain to the present day, in all their generality, and the remarkable phenomenon discovered by Mr. Faraday, on the optical modification of transparent substances, only tend to show that there also exists an action exercised on the part of magnetism, upon the molecules of other bodies besides iron, nickel, and cobalt. Thus the effect due to the action of magnets upon small needles of different substances are the same as the effects produced by the magnetism, 1st, upon the molecules of bodies; 2d, upon the molecules of iron, or magnetic particles, that are found mixed in a great number of cases. In the same memoir, I gave the relative value of the forces exercised, on the part of magnetism, on iron and on other substances, I found that the action of magnetism on iron, nickel and cobalt, being represented by 100,000, that which is exercised on transparent and tolerably pure substances, and which is variable from one substance to another, is comprised in general between 0 and 2. We may, therefore, admit as a mean, that the action of magnetism upon transparent substances is at least 100,000 times more feeble than the action exercised upon iron.—[A.]—The author had evidently not *read* Faraday's paper. C. V. W.

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BRITISH ASSOCIATION, 1846.

Decomposition of Water by Heat.—Prof. GROVE.

Mr. Grove first asserted, probably to the surprise of those in the habit of using the eudiometer, that every process that will combine gases can separate them. He was led to this result from the use of the eudiometer, mounted by him, which he briefly described; and by means of which he could apply intense heat to limited quantities of gases, analyze a bubble, even most accurately, or produce any effect of eudiometry; also decompose and analyze camphor, &c. Before proceeding to the experiments, he mentioned one point in relation to equivalents which he considered worthy of attention. It was that hydrogen, as pure as it could be obtained, passed into the glass tube of his eudiometer, and submitted to the intense heat of the platinum wire, contracted in volume. This he found to be due to a mixture of oxygen with the hydrogen; and whatever pains he took, the hydrogen was never entirely free from oxygen. Experiment proved this beyond a doubt. But the facts he observed, with the first and immediate reference to the discovery of the decomposition of water by heat alone, were, that two volumes, one each of hydrogen and carbonic acid, exposed to the heated platinum wire, contracted into one volume, the residue being carbonic oxide; and that carbonic oxide similarly exposed over water, expanded to one-third of its volume, this expansion being permanent, the carbonic oxide taking oxygen from the water and becoming carbonic acid. Here, then, were instances of oxygen being taken by hydrogen from carbonic acid, leaving carbonic oxide; and oxygen taken from hydrogen by carbonic oxide producing carbonic acid; an apparent case of the reversion of chemical affinity. If this were the case, Mr. Grove thought, as heated platinum wire combined oxygen and hydrogen to form water, it should also decompose water, and give off these gases. And this he eventually succeeded in accomplishing: first, a grain bubble only, but afterwards a current of decomposition; the platinum wire being heated as nearly as possible to the part of fusion by the electric battery. In order, however, to remove every doubt as to the possibility of the electrical action of the battery being concerned in the decomposition, he arranged an apparatus in which the heat of the blowpipe could be employed, and the same results were obtained, proving to his satisfaction that water was decomposed by heat alone. We have no time to describe the particular arrangements devised by Mr. Grove, nor to dwell upon his ideas of this discovery in relation to the spheroidal state of water, the possibility of its applicability to the arts, &c. Professor Faraday presiding, pronounced it a point of deep importance to the principle of chemical philosophy; and as Mr. Grove is again to make this, together with M. Schönbein's explosive cotton, subjects of discourse to an evening meeting, we may revert at our leisure to the minute details of

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the matter. We cannot, however, conclude without touching upon a point in the discussion that ensued, bearing immediately upon the theory of the fact.

Professor Playfair suggested that this decomposition of water may be a case of catalysis, and not as stated, the action of heat alone. Several metals, copper, gold, &c., at high temperatures, too, absorb oxygen giving it out in cooling. May not platinum, like gold, have a similar affinity for oxygen?

May not the gases separate by tension, and not by the pure action of heat alone? He would wish to see, instead of platinum, some substance tried that had no affinity for oxygen, to remove from his mind the impression of catalysis. Professor Faraday saw in the discovery a drawing near to the first actions of natural bodies. The question was, however, Whether it was a case of simple decomposition, or of the action of substances at high temperature having such affinities.—Whether the water gave way by heat alone, or by the powers of the body. He instanced euchlorine as an instance of the former. And it seemed likely to him that heat alone did it; and the results he thought may lead to a knowledge of the action of corpuscular forces. In regard to catalysis, Mr. Grove replied, that whatever bodies he used he may be met by the same objection. What substances could he use not capable of this action? What is catalytic action? Until this is explained we think the point may rest as a case of decomposition of water by heat alone.

Notices of Experiments in Thermo-Electricity. By Dr. J. Reade.—Some experiments were shown by which a brass bar, covered with paper, placed in the focus of a reflecting sheet of copper, bent into a semicircular form, and at a short distance from a spirit lamp was made to revolve This Dr. Reade thought to be due to the influence of thermo-electricity. Dr. J. Faraday, however, showed that a strong current of air circulated around the copper and the lamp, sufficient to take the smoke of ignited paper rapidly around; and thus, in all probability, the rotation must be referred to new currents of air, and not to any thermo-electrical action.

SOIREE ELECTRIQUE.

Sept. 18*th.*—According to annual custom, immediately after the meeting of the British Association, Mr. Gassiot gave a soirée at his house at Clapham, at which were congregated many of the lovers and the eminent cultivators of physical science. Among those present we recognised:—

Foreigners.

Professor Œrsted and Forchammer, "Copenhagen."
„ Svanberg, "Upsala."
„ Wartman, "Lausanne."
„ Matteucci, "Pisa."
„ Wappaus "Gottingen."
„ Roux, "Paris."

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Drs. Robinson (Armagh), Stevelly (Belfast), Reade (Cork), Fetton (Pereira), Miller, Rev. C. Pritchard, E. Bickersteth, the Lord Mayor, Ald. Hughes, Messrs. Grove, Wyon, Bowman, Laming, Hunt, Walker. Professors Richard, Phillips, Ansted, Playfair, &c. &c.

The battery of 100 Grove's was charged, and its splendid effects were exhibited. Many pieces of apparatus were on the table; also a splendid set of metallic chromes.

ROYAL ACADEMY OF SCIENCES, PARIS.

(*Continued from p.* 394.)

April 27.—*Researches on the Molecular Phenomena which accompany the production of the Voltaic Arc between two Conducting Points.* By M. De la Rive, Prof. of the Academy of Geneva. (pp. 4½.)

May 4.—*On the Universality of Magnetism.* By M. de Haldat.—A memoir, in which the author explains with great developments the experiments already made and announced by him in 1844 on the magnetism of all bodies. According to this work, bodies fashioned in elongated needles must be ranged into two classes; the first are composed of bodies which place themselves in the direction of the two poles of a horse-shoe magnet; the second class contains bodies which take a direction perpendicular to a line joining them at the same poles.

Electric Telegraph. Experiments made on the Rouen Line. M. Breguet, Jun.

As, for some time, much attention has been paid to the electric telegraph, many ideas, some false, others true, have been given by some journals, both as to the mode of propagating the electric current, and on the greater or less amount of power necessary to the electro-motive force in order to make the signal-apparatus act; they even threaten us with being obliged to employ eighty elements when we wish to work with an iron wire, I think it right to put philosophers in possession of the experiments that I am continually making on the Rouen line, both in a scientific as well as practical point of view.

M. Foy, director-in-chief of the telegraphic lines, thought that if several wires were arranged between two stations this would present the advantage, which, in certain cases, is very valuable, of being able to transmit two despatches in inverse directions at the same moment. This proposition, which certain precedents had authorized us to accept, was opposed by several persons.

It was of high interest to examine the question more closely; on this account, in connection with M. Gounelle, we endeavoured to transmit, at the same moment, on the line from Paris to Rouen, signals in inverse directions. The signals were presented on both sides with the most perfect accuracy. This experiment was repeated, and always with the same success.

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We repeated it again before the commission of the Chamber of Deputies, when, on April 7th, they honoured us with a visit; and on this occasion also, the success was as complete as it had formerly been.

It had been said that since with the copper wire we employed ten elements, it would require eight times as many, or eighty, when we made use of iron wire. The person who thus spoke was evidently not acquainted with the question. Guided by theoretical indications, I arranged a fresh apparatus, and instead of eighty elements, we only employed six, a number which I might probably limit to four; but, at this moment, our experiment was interrupted by the erection of a second iron wire. We will revert to them again as soon as this work is terminated, and I shall have the honour of rendering to you an account of what we shall do afresh.

I must not neglect telling you that we made a signal apparatus act through a distance of 400 k.m. (248½ miles), by connecting together the two copper wires and the iron wire already erected. The current was obtained from twenty elements.

Another experiment, no less interesting, is that in which we transmitted signals from Rouen to Paris with a single element. This fact seems to me important, inasmuch as it replies victoriously to the attacks, more or less unjust, directed either against the bad insulation of the conductors, or the want of sensibility in the apparatus; for, in order that a single pair at the end of a conductor of 137,000 metres (86 miles) should send enough electricity to make a small machine act, it seems that no great quantity must be lost in the course, and thus we must assert that the insulation of the conductor is good, and the instrument passable.

M. Wartmann describes experiments by aid of which he has succeeded in making sensible, by *calorific phenomena*, the molecular modifications which the action of magnets produces in bodies.

Mr. Morse gives certain details on the *electric telegraph* lines that now exist, or that are in the course of erection in the United States. The line from New York to Wellington will be terminated this spring; those from New York to Boston, and from Albany to Buffalo were likewise very much advanced at the end of 1845. The sum of the distances on these lines will be about 290 leagues.

Mr. Morse, in the same letter, speaks of the rapidity with which the apparatus that he has devised for tracing the signals operates. At the period when the Academy first entertained this apparatus, they had succeeded in writing sixty letters per minute, they now attain, in the same period, to ninety-eight, as appears from the experiments detailed in a pamphlet published by Mr. Vail.

NOTICES, &c.

Electric Telegraph Company.—By an Act passed on June 18, 1846, a company is formed for the more effectual carrying out of the principles of electro-telegraph. This Company have possessed themselves of Cooke and Wheatstone's patent, and are possessed of

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many important papers. The following is a general sketch of the lines of telegraph now existing in the country, which is independent of those at this moment in hand :—

	Miles.
South Western	99
South Eastern	132
Eastern Counties, Colchester	51
,, ,, Blackwall	3
,, ,, Cambridge	88
Norfolk Railway	38
Yarmouth and Norwich	20
Wolverton and Peterborough	57
Midland Counties, South Branch	49
,, ,, West Branch	41
,, ,, Derby and Lincoln	41
,, ,, North Branch	73
,, ,, Sheffield Branch	5
York and North Midland	23
Hull and Selby, and Milford Extension	40
York and Scarborough	43
York and Darlington	45
,, ,, Richmond Branch	9
,, ,, Newcastle and Darlington	39
,, ,, Durham Branch	2
,, ,, Shields Branch	3
,, ,, Sunderland	5
Preston and Wyre	20
Great Western	19
South Devon	20
Total	964

In this list only those lines are included which are actually in operation. A considerable addition would have to be made were the total lengths of the various lines now only in progress, or in course of erection, to be reckoned. Assuming a rough average, there have been used in these lines about 300 tons of iron wire, and about 5000 loads of timber.

Lightning Conductors.—Dr. Ferguson in the *Pharmaceutical Times* of Saturday, says :—"I have been desirous for some time of expressing my feelings relative to an absurdity under the garb of science, which disfigures an otherwise passable monument of art in this city. I allude to the lightning conductor which transfixes the bronze effigy of the Duke of York. Now as the statue is all bronze, I should like to know what can be the use of that ugly, pointed iron rod which projects through the head of his royal highness like the spit through a dressed turkey? Surely, if a metallic communication be established between the statue itself and the ground, so unsightly an addition must be unnecessary. Our works of art, on account of their architectural blunders, are already the laughing-stock of foreigners. We cannot desire any adjunct of false philosophy to add another eye-sore to our list."

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